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THE

EDINBURGH NEW
PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE
PROGRESSIVE DISCOVERIES AND IMPROVEMENTS
IN THE
SCIENCES AND THE ARTS.

CONDUCTED BY
ROBERT JAMESON,
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APRIL....OCTOBER 1838.

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THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL.

Biographical Memoir of the late Friedrich Hoffmann, Professor of Geology in the University of Berlin.

An acquaintance with the personal relations of an author often facilitates the use of his works, especially when the facts narrated as to his life have been collected with impartial diligence, and are represented with fidelity. Scientific writings more particularly, acquire much additional weight, from the confirmation they may receive from a knowledge of the life and character of their author. In the case of posthumous works, there is, in addition to this consideration, a wish to preserve, along with the fruits of the writer's mind, recollections of the natural powers and the means of cultivation by which these fruits were produced. No further apology need be offered for prefacing Friedrich Hoffmann's posthumous works by an account of some of the most important events of his short life; and it may be mentioned that the biography here given is partly derived from an article published by a friend only twenty days after his death.

Friedrich Hoffmann was born on the 6th June 1797, on the Pinnau, near Welau in East Prussia. His birth-place contained a number of mills of various kinds, at that time under the superintendence of his father, who, born in Silesia, and having had the advantage of a scientific education at a gymnasium, and afterwards at an university, commenced this occupation in the year 1791. He left the place in May 1798 and went to Königsberg, where he obtained a situation in the service of the state, that eventually caused him to remove to Berlin, in which capital he has acted as director of the statistical department since its establishment in the year 1810. It was necessary to mention these circumstances respecting the changes of residence and occupation of the father, as they influenced very materially the education and destiny of the son. The latter received instruction in the lower and middle classes of Friedrich's College, from the time he reached a proper age for school until the period when the family quitted Königsberg. His father taught in that institution from 1798 to 1807; and, under the direction of Counsellor Waldt, much time was there bestowed on the sciences, as well as the ancient languages. Friedrich Hoffmann early exhibited great facility in remembering facts and events, and in communicating to others the information he had acquired. The searching after plants and insects, whose names and properties he had learned at school, or accidentally from his father, seemed even then to occupy him more earnestly and uninterruptedly than it did his fellow scholars, who, however, were also generally pretty much interested in such pursuits. After the beginning of the year 1809, Hoffmann, now removed to Berlin, attended the *Friedrichs Werderische* Gymnasium; which institution was selected owing to the remoteness of the other academies from his father's house; but, while there, he was not among the most distinguished pupils. His taste for searching after and examining plants and animals had now become so preponderating, that it essentially interrupted the study of the languages, which ought to have been his chief occupation; the instructions in mathematics were also neglected, and the deficiency thus produced was only supplied at a later period of life by the assistance of a friend.

In February 1813, Hoffmann's elder brother obeyed the royal summons, and joined the companies of volunteer rifles, then being formed at Breslau; Friedrich remained most unwil lingly at the Gymnasium, but he had not yet reached an age included in the call to join the army. After the passage of the French over the Elbe, Berlin was no longer deemed a safe residence for the superior government authorities; and Hoffmann's father received an order to repair to Breslau with the other official individuals connected with the statistical office, and proceeded there with his family. He had scarcely reached Breslau when he was ordered to leave it, and remove to Landsberg on the Warthe; but his family remained behind, and Friedrich Hoffmann now entered the volunteers. Although at that time only sixteen years of age, he was, at his own request, admitted into the rifle company of the second battalion of the guards, in which his elder brother also served. After the announcement of the armistice, he accompanied his corps to Dresden, Leipzic, and Frankfort on the Maine. His brother then left him, having, with many of his comrades, been raised to the rank of an officer, and transferred to the troops which were destined to occupy again the fortresses of the Elbe. His father was brought unexpectedly to his neighbourhood, for, in the middle of December 1813, he was commanded to accompany the Chancellor of State to Frankfort, and afterwards still farther. The father and son met at Freiburg in Breisgau, and again at Basel, where, on the 12th January 1814, the guards crossed the Rhine. From this time the two were always so near that the communication of intelligence was easy and rapid; and the father learned that his son had been attacked by dysentery after the battle of Brienne, and obtained permission to have him brought to be under his own care, instead of being sent to an hospital. Although but little could be done for the sick in the unsettled kind of life that was then necessarily led, young Hoffmann soon recovered. Ere he was again fit for service, the war took a turn which rendered it impossible for him to rejoin his own company. While the guards, with the chief army of the allies, were directed towards Paris, the French army under the command of Napoleon himself, broke up the communication between the army and the diplomatic headquarters, which latter were withdrawn southwards to Dijon. Here intelligence was received of the entrance of the allies into Paris, to which capital the road was soon opened for the official authorities who had taken refuge in Dijon. Soon after his arrival in Paris, the father requested the discharge of his son from military service, which was willingly granted, as the war seemed to be ended. Friedrich Hoffmann remained two months with his father in Paris in the enjoyment of manifold instructive and exciting relations, and then hastened back to Berlin to continue his interrupted studies.

As a student of the first class, but not yet nearly prepared for the University, he had quitted the Gymnasium in May 1813; but no doubt existed in the minds of his father and teachers that he could not again return to it. The events of the thirteen months that had since passed over his head, had tended so much to the development of his mental faculties, that he had completely outgrown the forms of instruction belonging to a school. The want of a complete *Gymnasial* education was therefore first of all supplied by private instruction, until there was evidence of his fitness for entering the university. In autumn 1814, he was received as a medical student. The medical department was selected, because it is most nearly connected with the natural sciences, and because it offered the possibility of procuring a respectable situation in life, should no opportunity occur of obtaining an appointment as a naturalist.

The war, which recommenced after Napoleon's return from Elba in 1815, very soon interrupted Friedrich Hoffmann's medical studies. He was appointed officer in a regiment of the *Landwehr*, which, however, only advanced as far as the Weser, as the sudden and decisive termination of the hostilities brought about by the battle of Waterloo, rendered its appearance on the scene of war unnecessary. Hoffmann was thus able to obtain permission to leave the service before the end of June 1815, and now once more dedicated himself entirely to his studies, which he prosecuted in Berlin until Easter 1818.

At that time he formed a wish to spend a year at another university, and Göttingen was the one he selected. However accidental and slight might have been the causes in which this wish originated, its fulfilment exercised a very important influence on the direction afterwards taken by Hoffmann's studies. At an early period, botany was the science that attracted him the most; as he advanced in his medical studies a preference for zoology seemed to be awakened; but at Göttingen both these branches of knowledge began to be subordinate to a more general object of interest. He acquired his first ideas of mineralogy and geognosy from the lectures of Hausmann. The magnificent views of the history of the planet we inhabit, which geognosy at that time had partly unfolded, and partly only gave promise of revealing, thenceforward completely occupied the active mind of Hoffmann; he directed all his scientific investigations to these geognostical views, and his interest in them was never afterwards weakened. An interruption of the academical lectures in the summer of 1818, caused by disturbances, was much more favourable than the contrary to this change of the nature of his pursuits, for it afforded him leisure to seek out in Nature herself, as displayed in the Hartz, spontaneous proofs in favour of the instructions he had received, or reasons for doubting their accuracy. Hoffmann devoted the succeeding winter session chiefly to the formal completion of his medical studies, including his examination.

In the beginning of April 1819, when, in high spirits, and fluctuating between several plans for the future, he returned to his paternal home, he unexpectedly found the family in the deepest distress. His beloved mother had been buried the preceding day, and he had not even heard of her illness, so sudden had been the passage from apparently perfect health to the grave. This melancholy event decided him to remain for the present in Berlin, in order to console his bereaved and afflicted father. He found there much assistance to his studies in the public establishments, in intercourse with young friends who had a similar bias to the natural sciences, and in the free and unfettered leisure which he employed in beginning the regular study of mineralogy, under the guidance of the celebrated Professor Weiss. Anxiously striving to attain above every thing clearness in his ideas, and accuracy in his knowledge, he was by no means satisfied merely with the conclusions deduced by others, or with the examination of the specimens collected by others; he eagerly grasped every opportunity of making observations and discoveries for himself; and he did his utmost to obtain possession of the auxiliaries for his investigations, in order that he might not only make use of them, but also destroy them, if necessary, for experiments. He had already at Gōttingen formed a mineralogical collection, which contained more than could have been expected from his position as a beginner, the shortness of the time, and the limited extent of his means.

The more he advanced in knowledge at Berlin, the stronger became Hoffmann's desire to enter on some field for personal observations, which should be more productive for his studies than the Mark Brandenburg. As at first he did not possess the means of making a longer journey, he again directed his course, in the summer of 1820, to the nearest mountains—the Hartz—without having any very special object in view. But he had hardly crossed the Elbe, and traversed a portion of the hilly *Vorland* between Magdeburg and Helmstädt, when his researches assumed a definite and fruitful direction. Astonished by the sight of the alternation of the numerous mountain-rocks occurring there; combined with the remarkable phenomena of the parallelism of the lines of direction and the position of the strata, regarding which some of the more remarkable facts were then unknown: the thought struck him, that the study of the newer rocks, which previously had been much neglected in Germany, viz., of their geographical distribution in the north-western portion of his native country, and their connection with similar formations in England, would fill up a great blank in the science. This idea was the germ of all his subsequent investigations in that district, and he followed it up indefatigably for eight years. It is undoubtedly a striking trait of his truly scientific character, that, with all the ardour of youthful enthusiasm, he still not only was attracted by remarkable and important phenomena, but that, dwelling even upon those appearances which were obscure and concealed, he could not be satisfied until he had unravelled all the relations to his satisfaction. He felt that it would be of importance to devote a considerable period entirely to the special geognostical examination of a portion of Germany but little known; and for that purpose he visited during the following year the districts near the Weser, and the tracts of country lying between them and the outer extremity of the Hartz Mountains. With the view of furnishing as perfect a map as possible of the distribution of the formations, and thus enabling the individual observers resident in the country, and who were fixed at home, to compare the geognostical relations of their neighbourhood with those of remote quarters, he followed perseveringly the boundaries of the rocks, and at first bestowed but an inferior degree of attention on the internal structure of the formations. In order that he might not in any instance pass over the connection, even of the apparently most trifling geognostical relations, he combined in the year 1822 his observations on the Weser country with those of the first year; he also made an expedition to the isolated gypsum hills of Lüneberg and Segeberg, and another to the island of Heligoland, a spot so interesting in many respects, and whose geognostical relations were previously quite unknown. In the course of the following year he directed his attention to the districts lying south-west from the Hartz, and formed a map of the western boundaries of the high plain of Thuringia, the copper-slate mountains of Riegelsdorf, and the neighbourhood of the Meissner.

In the mean time, the investigations of Hoffmann in the hilly land lying to the north of the Hartz, had attracted the notice of Leopold von Buch, and as an honourable proof of the value of our author's first work, it may be mentioned that its publication was caused by the influence of that celebrated geologist.

That portion of the ministry to which the direction of public instruction in the Prussian States is intrusted, was now attracted by the investigations of Hoffmann, investigations which had also elicited the approbation of Alexander von Humboldt; and he was received as a private lecturer in the University of Halle. The circle of his hearers was but small, as few students had leisure enough to permit their acquiring knowledge which did not tend directly to their advancement in their particular professions; but nevertheless this circle increased gradually, and was characterised by great zeal and enthusiasm. The consequence was his speedy appointment as extraordinary professor at Halle.

But while occupied with his duties as teacher, the examination of the geognostical relations of north-western Germany was still his chief scientific occupation. Each year he employed the long summer vacation in examining thoroughly that tract of country; the field of his observations was at the same time enlarged in its internal contents, as well as external limits. Hoffmann extended it westwards to the slate mountains of the Rhine (1824); he devoted his attention likewise to the north-western branches of the German hilly country, between Münster and Bentheim (1825); he afterwards investigated the Erzgebirge and the Fichtelgebirge (1826); and finally he included in his survey the districts between the Hartz and the Thüringer Wald (1827). The range of connected observations was terminated after eight years methodical investigation. From year to year, he had already delineated the results he obtained on maps, but these now required an explanation; and it was necessary that the whole treasures of information acquired should be transferred as common property to the science of geology.

Leisure was now given him by the minister for public instruction, to prepare in Berlin, from 1827 to 1829, his great work entitled "*Uebersicht der orographischen und geognostischen Verhältnisse vom nordwestlichen Deutschland*" (General View of the Orographical and Geognostical relations of North-western Germany), Leipzic, 1830, accompanied by a geognostical atlas. The territory over which the investigations extended, amounts to not less than 650 square geographical miles; and a glance over the beautiful general index map, at once shews an extraordinary complication of extremely difficult relations. It was only by dint of indefatigable industry, and entire devotion to the object, that a single individual could, in so short a time, have perfected so comprehensive an undertaking. It is founded on such a multitude of separate observations as could only have been collected by indomitable perseverance, and unremitting constancy. But in Hoffmann's case the mass of details did not interfere with broad and talented general views. His connected and comprehensive representation of the form of the surface of the country, which was the result of more than 2000 measurements of heights, and his descriptions of the most intricate geognostical phenomena of that complicated district, were characterised by the most luminous order; and all bore distinct relation to the general question of the formation of the earth, a subject which received not unimportant contributions from the investigations of which we are speaking. Werner had only included the Thuringian secondary strata in his system, and there was still much wanting to render our knowledge perfect of the secondary formations of the north of Germany. Hoffmann contributed greatly to this knowledge; for he not only almost doubled the number of formations, but these were now for the first time accurately determined, and it was only now that their comparison with the secondary strata of other districts and countries was rendered practicable. We have to thank his talented ceaseless diligence for an accurate acquaintance with a considerable portion of our native country, and for a model of the manner in which further similar investigations ought to be prosecuted. Had he not contributed more than this single work, his name would have been assured of honourable remembrance so long as the German nation should place value on the knowledge of their land, and so long as the science of Geology should regard with interest an accurate detail of the structure of a portion of the earth's surface.

The mass of materials collected was, however, by no means exhausted in this work; there was enough remaining of most accurate notes, regarding detached important observations, to furnish matter for a second volume; but this required too much patience on the part of our eagerly aspiring author. Investigation, even of the most fatiguing description, was to him a pleasure, but the task of writing, and putting his materials in form, was burdensome. He longed for another field of research, for a new employment to his increasing powers.

The study of volcanos has in recent times become of the greatest importance; that is, since Vulcanism has gained the victory over the Neptunism which prevailed so much at one time, and more especially in Germany; and, since the opinion has been adopted that the rocks formerly termed Primitive are the products of burning liquid matter, similar to the lavas of volcanos,—nay, that, bursting through fissures in the crust of the earth, they have caused those disturbances and elevations in the secondary strata, which, without such an assumption, would be inexplicable. In this department, Leopold Von Buch had already obtained an imperishable name; and Hoffmann wished to become acquainted at least, with volcanic phenomena.

By the kind assistance of the ministers of the crown, he obtained the necessary means and leave of absence, to enable him to make a scientific journey through Italy and Sicily. The duration of the expedition was at first limited to two years, but was prolonged to three years and a half. He started from Berlin in October 1829, and did not return 'till March 1833.

Hoffmann formed the plan for his journey with the greatest circumspection, and made it as comprehensive as was permitted by his resources. It was of importance to him to arrive in the volcanic territory in possession of a perfect acquaintance with the language of the country, and of as extensive preliminary knowledge as possible; and provided with good instruments, and influential letters of introduction. For these purposes, the preparations he had made at Berlin were insufficient. He proceeded, first of all, to Vienna, where he remained till the latter part of January 1830. This prolonged stay in a capital so closely united with Italy, was for him a very rich source, not only of information in facts and the language, but also of very valuable acquaintance with individuals; even his Imperial Highness the Archduke John furthered his views by giving him the most important letters of recommendation. The winter of 1829–30 was characterised by greater cold, and a larger quantity of snow, than any previous one subsequent to that of 1788–9; it was particularly severe in the south of Europe, and Hoffmann, who entered Italy by Trieste and Venice, did not experience the perfect spring of the south till he descended from the Apennines into the valley of the Arno. His winning manners every where gave effect to his introductions; and from Florence he had the advantage of visiting, in most instructive society, the Maremma and the remarkable island of Elba. A lengthened visit to Siena was employed in writing an account of his observations, while they were still fresh in his memory, and while he was in a neighbourhood which rendered it easy for him to supply any deficiency. He left Siena about the middle of April; but the road to Rome presented so many objects of interest, that he did not arrive there till May. Two months were requisite to examine with care the classical site and vicinity of Rome; and his trips to the neighbouring mountains led to the idea of deviating from the direct road to Naples, and penetrating, during his journey southwards, into the Abruzzi. This tour rewarded him amply, but was unfavourable for the object he had in view, which was to arrive in the volcanic district at a good season; and it was not till August, the tenth month after his departure from Berlin, that he arrived in Naples. Well aware how far Etna surpassed Vesuvius in phenomena connected with his special object, he confined himself for the present to a cursory general view of the volcanic neighbourhood of Naples, and hastened onwards, in order to arrive at Catania ere it should be too late to ascend Etna. He therefore embarked, in the month of September, with three companions, at Naples for Messina; and he employed the remainder of the year 1830, in the first instance, in investigating the coast between Messina and Catania; and then, in ascending repeatedly Mount Etna, in traversing the wide extent of its declivities in all directions, in investigating, by dint of prodigious exertions, the most impassable ravines and inaccessible precipices, so that he might obtain a satisfactory knowledge of the peculiar features of this remarkable mountain, whose accumulations of ejected matter, formed during early single eruptions, sometimes surpass in extent the whole of Vesuvius. From Christmas till the middle of February 1831, violent torrents of rain prevented the continuance of his expeditions from Catania; but this leisure was necessary, in order to arrange his great mass of observations, and to dispatch the collections which he had accumulated. When the weather improved, he commenced the construction of a geognostical map of Sicily in its whole extent; and this great work, which was carried on with inexhaustible care and perseverance, occupied a period of more than ten months. Catania, and still more Palermo, served as resting points for the arrangement of his observations and collections. Hoffmann enjoyed also here the valuable assistance of distinguished patrons and well-wishers. A remarkable interruption to these extensive labours was caused by the unexpected breaking forth of a new volcano, which, at the beginning of July 1831, was raised up from the bottom of the sea, at a distance of a little more than five miles from the south-west coast of Sicily. Its erupted matter formed a hill of loosely heaped-up small fragments of slag, ashes, and sand, and which had a diameter of about 800 feet, and was upwards of 60 feet in height. Hoffmann, the moment he received intelligence of this rare phenomenon, hastened to Sciacca, the nearest sea-port, and, on the 24th July, in company with his companions in a small boat, he approached the new volcano as closely as could possibly be allowed by the rapidly succeeding eruptions. Even Sicily received the first accurate description of this natural phenomenon from our author. The voyage in the portion of the Mediterranean between Sicily and Africa, caused by this occurrence, was extended to Pantellaria, lying more towards the African coast; and that small, entirely volcanic island, was carefully examined. On the 25th September, the party visited, for the second time, the new volcano; which, in the mean time, had been extinguished, and could, therefore, now be investigated close at hand. The storms and waves combined to destroy the newly formed island; and, in the following winter, it entirely vanished beneath the sea. Hoffmann's geognostical labours in Sicily were concluded in Christmas by a voyage to the Lipari Islands, where he was detained for some time by contrary winds. Finally he sailed from Messina to Naples in February 1832, after a residence of seventeen months in Sicily.

He returned just in time to witness an eruption of Vesuvius, which commenced on the 22d February 1832. It was most fortunate that this eruption, on the one hand, was considerable enough to afford a clear example of such phenomena, and yet, on the other, was so moderate as to admit of a very near approach, which, if not free from danger, was at least not extremely perilous. The interest with which Hoffmann regarded the wonderful and classical country of Lower Italy, was immeasurably heightened by the information he had acquired, during his extensive and minute survey of Sicily. He looked with longing eyes to Calabria, and to the Greek islands, whither so many temptations invited him, and where his newly acquired knowledge would have opened the most inviting paths. It was only the unavoidable necessity of returning to his official duties in his native country that forced him to make the ruins of Paestum the limit of a journey so replete with enjoyment. As, with a heavy heart, he tore himself from Naples in the month of

August, he compared the light of Vesuvius to the fiery sword of the angel which drove our first parents from Paradise.

He proceeded by sea from Naples to Leghorn; and the gracious reception he experienced from the Grand Duke of Tuscany, combined with the varied information he received from his numerous acquaintances at Florence, Pisa, and Siena, delayed his departure till October. It was impossible for him to pass so near to the remarkable marble quarries of Carrara, without investigating them minutely. Thus the winter found him still in Upper Italy; and it was only when his researches were interrupted by the weather, that, about the new year 1833, he crossed the St Gotthard, and found a first resting-place, on this side the Alps, in the hospitable house of his faithful travelling companion (Escher) at Zürich.

Hoffmann was gifted by nature not only with a high order of mental powers, but also with a strong bodily frame, which his early campaign had rendered capable of great exertions. Already during his wanderings in northern Germany, his insatiable thirst for investigation, even of the most difficult country, had sometimes exacted too much from his great strength of body. He had suffered from some severe falls; and, in the hours of depression before his journey to Italy, he complained that his strength had begun to leave him just at the time when it was most required. Animated by the new prospects and enjoyments which the Italian journey presented to him, his mind gave wonderful support to his body. The necessity of seeing every thing with his own eyes, caused him to shun none of that exertion by which personal observation must generally be purchased. Although his bodily condition became somewhat precarious in Sicily, still he allowed himself no rest. The transition from the warm south to the cold north, is even perceptible in summer; but it is much more so during a journey made northwards in the winter season. The passage of the Alps, performed in the beginning of January, completed the exhaustion. Hoffmann came to Zürich in a state of great weakness; but a most friendly reception, and the most sympathizing care and attention, succeeded in restoring his strength so far as to enable him to travel to Heidelberg and Frankfort, and to reach Berlin about the end of March 1833.

Arrived in the Prussian capital, it did not for a moment seem doubtful to him, that the regular life of an academic teacher, and the cessation from great bodily exertions, would perfectly restore his health. He still retained his extraordinary professorship at Halle, and had only obtained leave of absence for scientific purposes. The richer auxiliaries for his studies presented in Berlin by libraries, collections, and, above all, personal intercourse with its men of science; together with the more extensive usefulness he might anticipate as a teacher, made him desire to be removed to the university of the capital. This was, with some difficulty, accomplished; and Hoffmann, in consequence of the improvement of his health, was enabled, during the summer of 1833, to prepare himself for commencing his instructions in Berlin in the winter session of 1833–4. The duties of his professorship now absorbed his whole time and strength; and all the incitements derived from the state of the science itself, all the urgent persuasions of his friends, failed in producing a division of his time and strength, which should render it possible to come forward as a writer, while he performed his duties as a teacher, and should thus enable him to render available, for a wider circle than his students, the fruits of his residence in Italy. But, however much he felt the necessity of making public his observations, yet he regarded his duties to his hearers as more imperative, and delayed all other claims on his time to a future opportunity, which unfortunately was denied to him. But the result of these lectures was most successful; it was his enthusiasm for the science, the clearness and perspicuity of all his demonstrations, and the warmth of his natural eloquence, which delighted those who listened to him. Language flowed from his mouth like an inexhaustible stream; a crowd of the most apt expressions stood ever ready at his command; and to every one it became apparent, that he had real pleasure and satisfaction in communicating the truths of his sublime science, and in acknowledging the services and discoveries of others. In his hands every subject acquired fresh life and interest; he invariably rivetted attention by the riches of his well-selected materials, and by the clearness and beauty of their arrangement. He obtained for his subject an unusual degree of interest on the part of his students, who seemed to be hurried along by the zeal of their professor. His house was ever open to his pupils; he gave up to them almost his whole time, and, whenever he found a fondness for the subject, he used every endeavour to secure young votaries of the science; although his career as a teacher was short, yet assuredly the matured fruits of his exertions will be visible in time to come. Hoffmann's academical life at Berlin embraced but four sessions: during the two winters he lectured on physical geography, and on volcanos and earthquakes; and during the two summers, on geography, together with fossils and hydrography. His deeply meditated plan was to extend his subject, so as to include general natural history, in order to unite with the successive epochs which indicate the history of the formation of our globe, the characteristic successive generations of plants and animals, and to pass from the original primitive inhabitants of the earth to those of the present time.

The apparent signs of a progressive improvement of his bodily condition, unfortunately caused him to overlook how much in his case precautions were necessary. He expected especially much advantage to his students from the geognostical excursions he proposed to make yearly,—at one time to the Hartz, and at another to the Erzgebirge. An expedition to the Hartz was proposed as the first of the series, and it was fixed for August 1835. Hoffmann felt himself indisposed in the spring of that year, but he considered the sensations he experienced merely as the consequence of a want of such exercise as he had been accustomed to for so many years during his wanderings and journeys; and he longed so much the more for a trip to the mountains. With this feeling of illness, and the desire for a cure, he went alone to the Hartz during the Whitsunday holidays, in order to select the route which might prove the most instructive. A map, which he intended should facilitate the comprehension of the phenomena to those who might accompany him, was already engraved; but the excursion never took place. Whitsunday week, 1835, was remarkable for its great heat compared to the otherwise cold summer. Hoffmann, accustomed to perform long daily walks, believed himself still in possession of sufficient strength for the purpose, and returned to Berlin fatigued by excessive exertions during intense heat, and depressed by this convincing proof of the diminution of his bodily strength. After this last effort, his vigour decreased with perceptibly greater rapidity. A hoarseness, which had already been apparent in winter, was now aggravated; and, with the bitterness of disappointed hopes, he himself confessed the urgent necessity of hastening to an end his summer lectures, of renouncing his proposed expedition, and of betaking himself to a watering-place; and, for this purpose, Ems was selected by his medical friends. The autumnal weather was no longer favourable for the baths; and, indeed, the internal disease had by that time advanced too far to yield to such treatment. Much reduced in strength, and evidently in worse health than when he left Berlin, he returned thither. He still cherished the belief that repose, a carefully regulated diet, and the refraining from exertion of mind, would reinstate his strength; and this belief did not forsake him even when he became daily weaker, was confined to his room, was compelled to have recourse to the constant use of a sofa, and finally became unable to quit his bed. He regarded these signs of a gradual, but steadily approaching dissolution, as only the consequence of the unfavourable season; and he was already forming a plan of waiting for the return of his strength, with the better weather, in retired summer quarters; and of occupying his leisure time in preparing the account of his Italian journey.

Since the middle of the summer of 1835, Hoffmann's friends had not been able to conceal from themselves the critical state of his health; their hopes, that his vigorous mind might still succeed in supporting his body, diminished every day, and by the new year of 1836, they had entirely given up all belief in his recovery. His illness excited a widely extended sympathy, even in the very highest circles of society. His mind remained serene and unclouded, even when his voice had become hardly intelligible. He expired on the evening of the 6th February 1836, in the arms of his younger and beloved brother, who had not left him for three weeks. The opening of his body proved how strong the frame must have been which was so late of yielding to a complete destruction of the most important internal organs. A numerous attendance of distinguished individuals and mourning students accompanied his corpse, on the

10th February, to the beautiful cemetery of the Trinity, where an iron cross now indicates his grave.

Friedrich Hoffmann's personal appearance was striking. A very successful bust, by the master-hand of Ludwig Wichmann, has preserved the highly expressive features of his countenance. His mind embraced, with ease and certainty, the most diversified subjects; not only was his attention drawn to natural phenomena, but also to manners and customs, manufactures, &c., and these branches found in him an anxious observer and impartial judge. A desire for the enjoyment of nature, and an inclination for social intercourse, were leading features in his disposition. The letters he wrote home from Italy were peculiarly characteristic of his mind and dispositions, and were particularly interesting, even when treating of well known topics. A catalogue of his writings is subjoined to this brief memoir. Science has to lament his early death; but assuredly his contributions and exertions were great, compared to the shortness of his career. His life was varied and active; upon the whole fortunate, and even enviable; he had not to wait for the approbation of posterity, but, on the contrary, enjoyed his fame during his lifetime. He possessed the friendship and regard of celebrated individuals of all conditions and occupations, both at home and abroad; his memory is secured of a place in the history of science.

Chronological List of Professor Friedrich Hoffmann's Works.

1821. 1. Attempt at a systematic arrangement of mountain-rocks, according to their natural relations. In Oken's Isis, 1821, part viii. p. 710.—1822. 2. On the Turf-moor at Linum; in conjunction with Chamisso and Poggendorf Karsten's Archiv, vol. v. p. 253.—3. On the occurrence of native copper in the island of Heligoland; Gilbert's Annalen, vol. lxx. p. 432.—1823. 4. Contributions to the Geognosy of Northern Germany. Part 1st.—5. Geognostical remarks on the Basalts of the Meissner district, and on their volcanic origin; together with a notice of some barometrical and electrical observations. Gilbert's Annalen, vol. lxxv. p. 323.—6. De Vallium in Germania boreali principalium directione memorabili congrua. Dissertatio inauguralis pro rite obtinenda facultate legendi.—1824. 7. Geognostical description of the secondary rocks which make their appearance at Lüneberg and at Segeberg, with a Notice on the direction of the rivers of Northern Germany and on the Lüneberger Haide, with a Map; Gilbert's Annalen, vol. lxxvi. p. 32.—1825. 8. On the geognostical relations of the left bank of the Weser as far as the Teutoburger Wald; Poggendorf's Annalen, vol. i. p. 1. 9. On the filling up of the cavities in Amygdaloid; Leonhrad's Zeitschrift, 1825, vol. ii. p. 490.—1826. 10. On the geognostical relations of the district of Ibbenbühren and Osnabrück; Karsten's Archiv, vol. xii. p. 264, and vol. xiii. p. 3. 11. Researches regarding the fossil plants of the coal-formation at Ibbenbühren and at the Piesberg near Osnabrück; Karsten, vol. xiii. p. 266. 12. On the copper-slate of the north-western portion of the secondary formations of the north of Germany; Leonhard's Zeitschrift, 1826, vol. i. p. 539.—1827. 13. On the volcanos of the South Sea Islands—being a preface to two memoirs by Ellis on the volcanic phenomena of Owaihi; Poggendorff's Annalen, vol. ix. p. 141.—1828. 14. On some newly discovered geognostical phenomena of the Plain of North Germany; Poggendorff, vol. xii. p. 109. 15. On Volcanic uprisings in the Moluccas; Poggendorff, vol. xii. p. 506. 16. Remarks on the volcanos of the island of Java; Poggendorff, vol. xii. p. 605. 17. Remarks on the occurrence of platina in the Ural; Poggendorff, vol. xiii. p. 566.—1829. 18. Remarks on the reciprocal relations of the ancient floras of the earth, with more particular reference to the *Prodrome* of Ad. Brongniart; Poggendorff, vol. xv. p. 415. 19. On the geognostical constitution of the territory of Rome, together with general remarks on the geognostical character of Italy; prepared by Hoffmann for the great work on Rome by Plattner, Bunsen, Gerhard, and Röstell; and also published in Poggendorff, vol. xvi. p. 1. *Translated in Edinburgh New Philosophical Journal, vol. viii. p. 76.* 20. On the relations of the crystalline rocks to the slates in the Hartz, and in the Erzgebirge and Fichtelgebirge; Poggendorff, vol. xvi. p. 513. 21. On valleys of elevation, and their connection with the origin of acidulous mineral springs; Poggendorff, vol. xvii. p. 151. *Translated in Edinburgh New Philosophical Journal, vol. ix. p. 349.* 22. Geognostical Map of North-Western Germany, in 24 sheets. 23. Remarks on the vegetation and fauna of Heligoland; published in the "Verhandlungen der Gesellschaft naturforschender Freunde zu Berlin," vol. i. p. 228.—1830. 24. General View of Orographical and Geognostical Relations of North-Western Germany, one vol. 8vo. 25. Series of Geognostical Plates of North-Western Germany. 26. Barometrical measurements of a line from the Saale near Halle, to the Weser near Höxter; Berghaus's Annalen der Erdkunde, vol. i. p. 48.—1831. 27. On the mountains of Albano and on Ætna, (with a view of the Val di Bove); Karsten's Archiv, new series, vol. iii. p. 361. *Translated in Edin. New Phil. Journ., vol. xii. p. 370.* 28. On the Temple of Serapis near Pozzuoli, (with a Plate); Karsten, vol. iii. p. 373. 29. On the tertiary formations of the coast of Sicily, the relations of the basalt to the west of Syracuse, and ossiferous breccias, (with Views); Karsten, vol. iii. p. 383. 30. On Cape Passaro and the Val di Noto, (with a Map); Karsten, vol. iii. p. 397. 31. Remarks on the New Volcano near the town of Sciacca, in a letter to the Duke of Serradifalco. Palermo, (in Italian). 32. Letter to Niccolo Cacciatore on the New Volcano, published in the Giornale di Scienze Lettere e Arti per la Sicilia, October 1831.—1832. 33. On the Earthquakes observed during the last forty years at Palermo, with reference to their direction, distribution according to the seasons of the year, and probable influence on the barometer; Poggendorff, vol. xxiv. p. 49. 34. A large portion of an article on the new volcanic island in the Mediterranean, published in Poggendorff, vol. xxiv. p. 65. 35. On the ossiferous grotto of Mardolce, near Palermo; Karsten, vol. iv. p. 253 (with a Plate). 36. On the geognostical constitution of the Lipari Islands, (with four Plates,) published in Poggendorff, vol. xxvi. p. 1, and also separately.—1833. 37. On the geognosy of

Massa Carrara; Karsten, vol. vi. p. 229. *Translated in the Edinburgh New Phil. Journal, vol. xxi. p.* 116. 38. On the volcanic formations of Naples, Sicily, and the Lipari Islands, in the "Bulletin de la Société Geologique de France," vol. iii. p. 170. 39. Observations on the communications made regarding Sicily by M. C. Prevost, ibid. vol. iii. p. 175. 40. Observations on the marble of Carrara and some fossils of the environs of Spezia, ibid. vol. iii. p. 179. 41. Observations made with M. Escher jun. on the porphyries of the southern flank of the Alps, in the Canton Tessin, ibid. vol, iv. p. 103, and 326.—1835. Geognostical Map of the country between Magdeburg and Kassel, intended for the use of his students during the projected excursion in 1835.

To this list there is now to be added the first volume of Hoffmann's Posthumous Works, from which the present memoir is taken, and which is devoted to a treatise on Physical Geography. The second volume will contain our author's views on the more general departments of Geognosy, and more especially on Volcanos.

Upon the Formation of Crystals in the Cells of Plants. By Dr F. Unger.

Some authors have erroneously maintained that the crystals which exist in plants are not to be found in the cellules themselves, but in the intercellular spaces. They may have fallen into this error from the fact that these groups of crystals are sometimes so voluminous that they very much disturb the cell, and give it a bulk of at least six times its natural size. In general, these crystalline cellules contain no other organic substances, though I have found in the *Piper blandum* crystals, mixed with numerous grains of chlorophylle, within the same cell. Meyen was the first to discover these crystals in the epidermic cellules, for, previous to his time, it had been supposed that they were confined exclusively to the parenchymatous cells. To the two plants, the *Maranta Zebrina* and the *Tradescantia discolor*, which he pointed out as presenting this peculiarity, I can now add a third, viz. the *Goodyera repens*. At the same time, it is true that it is usually in the parenchymatous cellules that these inorganic bodies are found. They appear in all the divisions of the vegetable kingdom, from the simplest algæ (*Nostoc muscorum*, *Conferva crystallifera*) to the highest organized vegetables. It seldom happens that only one crystal is found in the same cellule, though this peculiarity may be found in the *Papyrus Antiquorum* and the *Ficus Bengalensis*; but much more frequently, even in the plants we have named, each cell contains many. In those plants which have aërial cavities, the crystal cellules are often free, and project in the interior of these cavities; and this is especially true of the acicular crystals, as M. Meyen has demonstrated in his Phytotomy; and I have also observed it in the starry like crystals of the *Myriophyllum spicatum*. The existence of crystals seems to be connected with the cells containing fecula, and such like substances, such as resin, chlorophylle, &c. The vascular bundles, on the other hand, seem to have no apparent connection with the crystalline formations.

We shall first attend for an instant to the form of these crystals. This, their great minuteness makes it very difficult to determine, for nothing definite can be stated on this point, if we cannot determine the angles which the different surfaces make with each other. To effect this object, M. Raspail has invented his microscopic goniometer; but this instrument is too difficult to manage, since the limits of its errors may be less than the differences which exist between the different vegetable crystals. This will easily be conceived, if we reflect that the largest crystals which I have examined were not longer than 0,11 of a Vienna line; and a great number were only 0,023, and smaller than this down to their being infinitely small. The results which I have obtained on this point perfectly agree with those of M. Raspail. I have given figures, as accurate as possible, of the crystals I found in the following plants, viz., *Ficus Bengalensis*, *Maranta zebrina*, *Musa paradisiaca*, *M. coccinea*, *Yucca gloriosa*, *Strelitzia reginæ*, *Papyrus antiquorum*, *Tritoma uvaria*, *Aloë pulchra*. In these vegetables, the crystals are isolated, or at all events free in the interior of the cellules; often, however, they are grouped, and even soldered together, as may be observed in many *Rhubarbs*, also in the *Myriophyllum spicatum*, *Herniaria glabra*, *Mercurialis perennis*, *Viburnum Lantana*, *Cactus pendulus*, and the *Caladium nymphecæfolium*.

The chemical examination of these bodies does not present fewer difficulties than the determination of their physical forms. Buchner, Schübler, Saigey, De la Fosse, Nees d'Esenbeck, and Raspail are the only individuals who have engaged in the investigation. Their bases appear to be lime, magnesia, and more rarely, silica. With them the carbonic, oxalic, phosphoric, and tartaric acids combine. The process which I employed differed from that of Raspail. I first allowed the crystalliferous tissue to digest in diluted nitric acid; I then filtered, and added ammonia; I collected the white precipitate which was then thrown down, and heated it on a platina dish. Its effervescence proved the existence of some vegetable acid, probably the tartaric or oxalic.

The author subjoins, somewhat incidentally, some observations upon the *latex vessels*, in connexion with a figure which accompanies his memoir. Their purport is to the following effect. These vessels, described by Schultz and Meyen, are altogether different from the *vasa propria*, as M. Mohl has proved. They also contain a juice which is much more mucilaginous in its composition, as well as resinous and gum-resinous substances. The *vasa propria* constitute a part, so to speak, of the vascular bundles, and probably discharge an important function in the movements of the juices. The *latex vessels*, on the contrary, never accompany the other vascular bundles; they are invariably separated from them by cellular tissue; and develope themselves by the anastomoses of many cells placed end to end. In fact, they appear to me to belong more to the cellular than to the vascular system. In one of these figures the author has represented these nascent vessels as they appear in the pith of the *Ficus Bengalensis*. Here they are quadrilateral cellules placed end to end; nor do they differ from other parenchymatous cellules, unless it be that they already contain a great number of those roundish granules which form the greatest portion of the latex. This mode of formation is in perfect harmony with what we observe concerning the formation of other organs, such, for example, as the spiral vessels, which are nothing more than anastomosing cellules placed end to end. M. Unger has never hitherto been able to determine any movement of the *latex*, and he purposes soon to prosecute the investigation of the subject.

On the Classification and Antiquity of the Cheiroptera. By M. de BLAINVILLE.

M. DE BLAINVILLE lately read to the Academie des Sciences the result of his researches concerning the antiquity of th

Cheiroptera, or the animals of the Bat family, on the surface of the earth, premising preliminary observations on the scientific history of the subject, the principles of their classification, and their present geographical distribution. This memoir is extracted from a work the author means speedily to publish under the title of "A System of the Animal Kingdom, founded upon the whole organization, and its functions, as inferred from the external characters." Each great genus of the Linnean system will thus be brought under review, and the fossil species, as well as the existing ones, will be considered. We shall now present an epitome of this essay, whence it will appear, how, in the projected work, M. de Blainville proposes to investigate every group of the animal series.

The Bat is denominated by the Greeks Νυκτερος, and by the Latins *Vespertilio*; and both of these appellations designate it better than that of *Avis-Sorex* (flying mouse), of Oiseau Musaraigne, of Souris-Chauve (bald mouse), or, finally, of *Chauve-Souris*, now the general French name. According to M. de Blainville, this unfortunate denomination, which gives to the bat a kind of equivocal nature, is to be considered as the cause why naturalists have not assigned to these animals their proper place, according to their natural relations; and hence he prefixes the scientific history of these animals. He then points out the arrangement in which he proposes to place them, both as it regards their genera and species. We shall allow him, in his own words, to explain the principles which have regulated him.

As by a natural methodical arrangement is understood something stable, which reposes on the existence of an animal series, and in which, therefore, there is nothing arbitrary, it is clear that the zoologist only attains this object when the first species of a group is that which most nearly approaches the last of the preceding group, and when the last is that which is the least removed from the first of the succeeding one. It likewise follows, that when these two points are determined, the intermediate arrangements follow as a matter of course. Now, what essentially constitutes a bat, or rather the group of the Cheiroptera, the first order of the carnivora, is, that they fly more or less, that they may procure, and often that they may pursue their prey, and consequently, that they have a certain more or less striking disproportion among their members, and especially that of the anterior ones, compared with the trunk; and, secondly, that they are more or less carnivorous, and consequently have the dental apparatus more or less fitted for this purpose, or in other words, have the molar teeth closer together, more numerous, and beset with sharper tubercles. The regular arrangement of the bats in a series, then, must be founded, first on the degree of the development of the cutaneous expansions which enable them to fly, and of the parts which sustain them, as the anterior extremities in general, and their *digits* in particular, also the tail, which, by being prolonged more or less backwards, and beyond the feet, enlarges so far the *interfemoral* membrane, because, in fact, it unites the posterior members. Thus, in this point of view the first species considered will be those which, in proportion to the size of the body, have the smallest flying apparatus, and also the smallest tail; and the last species will be those in which the flying apparatus attains its greatest development, affecting in the same way the bony machinery connected with it.

The second portion of the organization of the Cheiroptera, which will assist in determining their natural methodical arrangement, is the state of the dental system, as it is found more and more carnivorous and insectivorous. Now, this character is determined, in general, by the presence of a greater number of teeth, and especially by the more acute arrangement of the tubercles upon their crowns. Hence a degree of importance which progressively increases from the incisors, —which exhibit many variations, not only in their number but in their form also, according to age and species, and to such an extent that they may quite disappear,—to the canines,—which are never wanting, but are more or less developed,—and especially to the molars, which must be studied in the most minute manner as to their number, their relative proportions, and also the number and proportions of their tubercles. After the minute study which I have directed to this portion of the dental system of the bats, I may mention I have only hitherto found five combinations, to which we may attach the names which M. F. Cuvier has attached to several.

1st, $\frac{4}{4}$ as in the Scotophilus $(\frac{1}{1}+\frac{3}{3})$
2d, $\frac{4}{5}$ .... Serotinoid $(\frac{1}{2}+\frac{3}{3})$
3d, $\frac{5}{5}$ .... Noctuloid $(\frac{2}{2}+\frac{3}{3})$
4th, $\frac{5}{6}$ .... Semi-Murinoid $(\frac{2}{3}+\frac{3}{3})$
5th, $\frac{6}{6}$ .... Murinoid $(\frac{3}{3}+\frac{3}{3})$

The consideration of the nasal *concha*, which is quite wanting in some species, and is most singularly complicated in others, as well as that of the auditory *concha*, equally remarkable for the various degrees of its development and complication, afford much more secondary characters in the natural arrangement of the bats, although they supply admirable characters for the distinction of species, but their expression is often difficult, even in drawings, on account of their shading away into each other in a way that is sufficiently troublesome.

The digital system of the anterior extremities especially, the tail, and the interfemoral membrane which it supports, constituting as they do an important element in the manner of the bats' locomotion, in truth supply characters of much greater importance than the olfactory or auditory concha, inasmuch as they almost always run parallel with the characters drawn from the two parts already mentioned.

Regulating himself by these suggestions, M. de Blainville is disposed to arrange the sub-order of the Cheiroptera in the following method :—*First*, the Roussettes, as has been done by all zoologists, as those Cheiroptera that approach nearest to the Galeopitheci which terminate the Makis ; and as comprehending the species which are by much the least qualified for flying, and are the least insectivorous, or in other words, the most frugivorous ; and at the other extremity M. de Blainville places the bats properly so called, as possessing the highest dermal development, the greatest want of proportion in the anterior extremities, and the greatest length of the tail and of the interfemoral membrane which accompanies it to the very extremity, whilst at the same time they possess the most insectivorous dental apparatus, passing thereby towards the small insectivorous carnivora, and amongst others towards the moles and hamsters.

The distribution of the species of the sub-order naturally flows from this arrangement. They are first divided into three families, the Roussettes or *Meganycteres*, the vampires or *Phyllonycteres*, and the bats or *Normonycteres*. The first family is formed of those which have the nose and ears simple, the two first digits or fingers complete, or very little deformed, with no tail or interfemoral ligament, or with these parts very short; the molars also are distinct and very simple. In the second family, the first digit alone is complete, the molars are more or less sharply tuberculous, and they have the nose more or less complicated at its orifice. In the third family the nose is constantly simple. The species of Roussettes are then arranged by beginning with the common Roussettes, which have the head and jaws most elongated, and terminating with the Cephalotes in which they are the least developed; and between these are intermediately comprehended the subdivisions named *Pachysoma*, *Harpia*, *Hypoderma*, *Cynopterus*, *Epomophora*, and *Macgroglossa*, which being nothing more than successive shades of difference without any influence on the manners and habits, should not, according to him, have been adopted as distinct genera.

The species of Vampires or Phyllonycteres, beginning with the Glossophaga, and evidently passing to the Macroglossa of the preceding family, and finishing with the Nycteres, which approach very near to the Taphiens of the third family, are divided into three principal genera; viz. the Stenoderma, whose tail and interfemoral membrane are still very short, as in the family of Meganyctera, comprehending the sub-genera, *Glossophaga*, *Desmodus*, *Stenoderma*, which last includes the *Dyphylla*, *Artiboeus*, *Madotæus*, and *Brachyphylla*; 2*d*, the Phyllostoma, whose interfemoral membrane on the contrary is very large, passing beyond the origin of the calcaneum, and the more Carnivorous species of which are still further divided in relation to their tail, which at first does not exist, and then becomes longer and longer in the three genera of *Phyllostoma*, subdivided into Vampyrus, Monophyllus, and Mormoops; and 3*d*, The Megaderma and Rhinolophus, subdivided into Rhinolophus properly so called, *Nyctophilus* and *Nycteris*.

The species of the Bats or Normonyteres, which are characterized by having a simple nose, and by the almost uniform existence of a long tail, are, from the consideration of this appendage, divided into three sub-genera, viz, 1*st*, Noctilio, in which the tail is engaged only near its base, and is free above the membrane throughout the remaining portion, is distributed into the two sub-genera, *Taphozous* or Taphien, and *Noctilio.* 2*d*, Molossus (E. Geoffroy) whose tail in the same plane as the membrane, is not accompanied by it at its terminal portion; and these may be subdivided from the consideration or absence of the small superior false molar, into *Molossus*, *Cheiromeles*, and *Myoptera* or Dysopes. 3*d*, *Vespertilio*, whose tail is wholly engaged quite to the extremity of the membrane; this group is composed of the sub-genera *Emballonura*, *Furia*, and *Vespertilio L.*; it subdivides itself into *Scotophilus*, Serotines, Noctuloides, and Murinoides, *Plecotus*, and *Nycticœcus.*

Having thus established the series of the Cheiroptera or flying Insectivora, as serving to connect in an evident manner the Makis or the last family of the *Primates*, with the moles and hamsters, which ought to commence the grand series of the Carnivora, the author shews, in treating of their present geographical distribution, that one of the branches of this family is confined to the hot countries of the old hemisphere, but that it belongs essentially to its insular parts, beginning in the Continent of Africa below Cairo, and terminating with the last of the Australian Isles: these are the Roussettes. Another branch, viz. the Stenodermes and Phyllostomes, compensate, as it were, for this, and in fact are only found in South America, whilst the remainder of this branch belongs exclusively to the ancient Continent throughout. These are the Megadermes and the other Rhinolophi. Finally, the last branch, that of the Bats, is found in every part of the world, and ascends farthest into the Arctic regions; but only certain species of the genus Vespertilio properly so called, and a single species of Molossus, are found in Southern Europe, and there is also but one species of Nycticœcus.

Passing now to the antiquity of the Cheiroptera upon the surface of the earth, M. de Blainville shews us that in the writings of Moses, bats are numbered among the impure animals which the Israelites were not to eat; and that, in hieroglyphics, they are employed to indicate a nurse or mother suckling her infant, &c. At the same time, it ought to be remarked, that the ancients have not left us any monument which expressly represents any living animal of this family, unless we put down to this account the figure which they frequently give of the Harpy, which, in reality to a considerable extent, resembles it. But if bats have thus left no traces of their existence upon the surface of the globe, except in the writings of man, it is very different in the successive layers of the crust of the earth, or in what is usually denominated the fossil state. Without particularly enumerating the different discoveries which have been made on the point, during the century and a half to which the attention of naturalists has been directed to this subject, we shall only notice that he sums up, nearly in the following terms, our present knowledge of fossil remains of the Cheiroptera. 1*st*, Animals of this family existed before the formation of the middle tertiary deposits of the northern or European countries, since undoubted remains are found in the gypseous formation in the neighbourhood of Paris. 2*dly*, These bats were probably contemporaneous with the Anoplotherium and the Palæotherium, as their bones are found in a similar geological position. 3*dly*, They have continued uninterruptedly to exist from that time to the present day, and that throughout the whole of Europe, because their remains are found in the diluvium of caverns and in osseous breccias. 4*thly*, That these very ancient bats do not differ much, if indeed they differ at all, from the species which are now living in these same countries. Hence it may be deduced as a legitimate result, that the conditions of existence which are essential to them now were the same at that epoch, more or less distant from the present time, and, therefore, that there has been no considerable change in the totality of the circumstances, or at least these changes must have been in the last degree insignificant, and in limits of variation such, that the *maxima* and *minima* oscillate, as they now do, without any appreciable influence upon organized beings.

M. de Blainville concludes by giving us a *synopsis* of the arrangement of the genera, including the principal species; but what we have said above may suffice for the comprehension of his classification.

On the Propriety of Forming National Casting Establishments, with Observations on the Improvements which would result to Science, Literature, and Taste, by the adoption of such a measure.[*] By C. W. Wilson, R. I. A., and A. S. A.[†] (Communicated by the Society of Arts for Scotland.)

Amongst the various subjects which occupy the public mind, there is no one more interesting than that of the nature and extent of the encouragement which an enlightened government should afford to the Fine Arts.

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[*] Read before the Society of Arts, 28th March 1838.

[†] Report of the Committee of the Society of Arts appointed at the Meeting 28th March 1838.

Wednesday April 4.—Present :—Sir Thomas Dick Lauder, Bart. ; Sir John Robison ; Mr William Fraser ; Dr Maclagan, Convener.

The Committee to whom was referred the communication by Charles Wilson, Esq., R. I. A., and A.S.A., "On the Expediency of Forming National Establishments for Moulding and Casting Works of Art," read at last Meeting of the Society, having heard Mr Wilson in further explanation of the object proposed, beg leave to repeat,—First, That it appears the formation of such establishments in this country was suggested by Mr Andrew Wilson, the author's father, now residing in Italy, and well known as a distinguished artist and man of taste, in a communication made to the Board of Trustees for Manufactures in Scotland in the summer of 1837.

Second, That the object proposed in Mr Charles Wilson's communication is deserving of the warmest encouragement and support of the Society, as calculated, by the facility it would afford to the diffusion throughout the country of the purest models for emulation and instruction, to raise the taste, and improve the execution not merely of artists, but of artisans and manufacturers; and at the same time to elevate the standard of public feeling, and create a demand for works of a superior class in every department of industrial art.

Third, That, impressed with these feelings, and with the conviction that this great national object can be fully attained only by the fostering aid of government, the Committee earnestly recommend that the Society of Arts should co-operate in every means for its advancement.

Fourth, That in furtherance of its success, the Society should address a Memorial to the Lords of Her Majesty's Treasury, praying their support and encouragement of the measure, with the expression of their confident hope, that in the event of a national establishment being formed in London, its benefits may be extended by the formation of a branch institution in this country, where casts may be obtained by native artists and artisans at a reasonable rate, and free of the cost of transport from moulds made in London from the finest casts in the Metropolitan establishment. Fifth, In examining this interesting question, and in devising means by which the arts may be fostered, encouraged, and directed, we are bound to study the history of the art in past ages in different countries; and by a consideration of the means by which it attained excellence in these, and a comparison of the different degrees of perfection observable in the various schools, the works of which are in high repute, we may arrive at some satisfactory conclusion as to the best means of encouraging the different arts of design amongst ourselves.

The schools of the Continent which have flourished at different periods are so numerous, that a full consideration of the means by which they arrived at distinction would extend our inquiries beyond a reasonable compass. However important such an investigation might be, we must at present confine our observations within as brief a space as possible. We shall therefore be contented with alluding to four well known schools,—the Roman, Florentine, Venetian, and Flemish.

The history of art proves that the patronage which was afforded to the principal artists of these distinguished schools, was of the most munificent description. Popes and other sovereigns, princes, nobles, and the wealthy of every degree, united to encourage art, and to honour and enrich its professors. The effects produced by a liberal and enlightened patronage of the arts in different countries, were the full development of native genius, and the attainment of different degrees of perfection in each of them respectively. As the four schools above mentioned were all powerfully and extensively patronised, a comparison between their respective merits, may lead to important conclusions as to the best means of fostering art.

Fifth, That the establishment in this country may be advantageously placed under the superintendence of the Board of Trustees for Manufactures in Scotland, to whose taste, judgment, and liberality, the public are indebted for the admirable collection of casts in the Gallery of their Academy, and for the free admission of all classes of the community.

Finally, Your Committee are persuaded that the formation of national establishments for moulding and casting works of art, conjoined with the free admission of the public to many of the museums and collections of the country, which they trust will be still further extended, would materially tend to raise the character, and improve the social qualities and condition of all classes of the people. DAVID MACLAGAN, *Convener.*

Although patronage thus led to the full development of genius, yet the paths followed by the various schools were widely different, and it is to this *fact* our attention must be turned. We perceive that patronage will effect a certain object; but that it will create a school of art distinguished for qualities of the highest description is another question. By a glance at the history of the four schools we have chosen, this question may be settled.

Does a school of art exist, distinguished for qualities with regard to the merit of which all competent judges agree?—unquestionably. The Italian, the German, the Frenchman, and Englishman, each differing materially in opinion with regard to which is the true path in art to be followed, have no difference of opinion with regard to the superior excellence of the Roman and Florentine schools. If all agree as to the merits of the great masters of these, we find considerable diversity of opinion as to those of the Venetian and Flemish, especially the latter; these last, however, are placed by common consent in a lower grade than those of Rome and Florence.

It is quite unnecessary on the present occasion to inquire into the nature of the qualities which give the Roman and Florentine schools so pre-eminent a place in our estimation. When we consider that the masters of these schools were surrounded by some of the finest works of antiquity, many of which were discovered in their time; when we reflect that they studied them diligently, and when we read in contemporary historians of the effect which such studies had upon their works, we feel that they owe the sublime excellence for which they *alone* are distinguished amongst modern schools, to the inspiration which the genius of their authors derived from the study of the immortal productions of the Greek chisel. Much as we may admire the exquisite powers of painting exhibited in the works of the Venetian masters, we cannot claim for them the same greatness of design, and poetry of conception, which so much overpower us with admiration when regarding those of the masters of the Roman and Florentine schools. The possession of dexterous manipulation in painting, for which the Venetians were remarkable, is too often apt to mislead the judgment, and consequently, we find that it contributed to the degradation of the art with them, as well as elsewhere. We cannot, however, refuse them an equality with their great rivals in beauty and harmony of colour. We must even concede to them, that owing to their system of painting, they excelled them in brilliancy; but we must again repeat, that in sublimity, poetical feeling, and all the qualities which touch the soul, they must yield the palm of superiority to their great competitors. None of the beings they represented rise in a great degree above the nature they studied, and their drawing is often incorrect.

The cause of this will be obvious to every one: the Venetian painters had no opportunity of properly studying the antique, and of purifying their design by such models; they indeed adhered to the nature which surrounded them, with a degree of strictness which frequently rendered their works incongruous, the costumes of their own times being applied to subjects taken from ancient history.

The Flemish school now comes to be considered, and it must be placed on a level greatly lower than that of Venice. It is a glaring example of the disastrous effects which may result to art from inattention to its higher principles. Possessing a genius, equal to any, perhaps, of those of the great Italian masters, having a fertile imagination, and unequalled powers of execution—Rubens—the great head of the Flemish school, contributed to the corruption of art and taste. His design is the very reverse of that which, by common consent, is held to be the only true model of purity; he copied the coarse and vulgar nature with which he was surrounded. His works are a proof that, when uninfluenced by the principles developed in Greek art, powerful genius, although stimulated by the most extended patronage, cannot rise above the surrounding level of natural objects.

These brief observations having now been made on the general characteristics distinctive of peculiar schools, and one predominant cause having been assigned for the superiority of the two former of these schools over the two latter, we consider it unnecessary to notice any of a lesser importance; the argument that this is the primary cause of this superiority, is that with which we are at present chiefly concerned. The inspiration, the soul-begotten qualities, distinctive of Greek art, are equally evinced in the productions of the Roman and Florentine masters; their art addresses the intellect. The Venetian school has been mentioned as deficient in the higher qualities of art, from the taste of its masters not being purified by the study of classic models; and Rubens has been prominently noticed as a strong instance of the errors genius may commit when unfettered by, or ignorant of, those rules which guided the taste of the Greeks in their immortal productions.

We may be allowed to glance at the influence of classic taste on *all* the arts of Italy, and at the results of the absence of this influence, especially in our own country. A traditionary taste existed in Italy, even in the darkest ages; it is evinced in all the productions over which art exercises control, in the paintings, in illuminated MSS., in the ornaments on coats of mail, in the utensils of common life, whether in metal or in other materials; and this traditionary taste derived from happier times, prevented in Italy the perfection of the universally spread Gothic architecture. When the discovery and study of ancient works led to the restoration of the arts, the Italian mind reverted with facility to the only true path in painting, sculpture, and architecture; classic taste re-asserted its sovereignty. After a time, art declined in Italy; an unhappy craving for novelty, and an insane desire of originality, corrupted it and utterly perverted taste. The example of the great masters was forgotten, and the precepts manifested in the works of the ancients neglected.

The success with which the arts were cultivated in Italy, exciting the admiration of foreigners, Italian art spread over Europe, and Italian artists were induced by prospects of high reward to visit foreign countries. These artists brought a style founded on the classic, to countries where all the existing examples of art were Gothic. It was quite impossible that the new style could eradicate ideas, associated with every monument of national importance, and accordingly the result was a mixture of styles utterly opposed in principle, which it is to be regretted has now many admirers, and still more unfortunately many imitators.

The absence of those pure examples, which in Italy pointed out the true path, and as long as they were studied kept art in it, is the certain cause why taste erred so greatly in every other European country.

In England, we find no traces of classic purity of taste, except in its architecture. Foreign professors of the sister arts imported the bad taste prevalent on the Continent, and nothing can be more melancholy than a retrospective glance at the state of our national art, till the period when it awoke to a better existence in the middle of the last century. But even then the art adopted as a model, was not that of the highest schools, and it cannot be asserted that what is termed *high art* has ever flourished amongst us. The Venetian and Flemish schools have exercised a fatal influence over us, and whilst we have many artists remarkable for their powers of colour and chiar' oscuro, we fear that we cannot boast one *great* historical painter.

Let us look back at the history of every thing connected with art in the last century; for instance, the costumes on our stage, and the various products of our national industry; and we may reflect with a feeling of shame on the ignorance and want of taste displayed.

English sculpture should have been mentioned before the foregoing paragraph, but its history brings us to a new era. The degradation of this art is painfully evinced in our Cathedrals. When classic art, however, became more an object of study, sculpture rapidly rose; the Elgin marbles have contributed immensely to its improvement, and sculpture now takes a prominent place amongst our arts; adding yet another to the many proofs of the advantage of studying and being guided by classic models.

In the late Parliamentary inquiries instituted regarding the state of the industrial arts in England, numerous witnesses examined by the committee attested the superiority of French taste in ornamental manufactures, and this has been chiefly attributed to the number of schools in France for the education of mechanics in the arts of design. The excellence of the models and of the examples set before the pupils has also been dwelt upon in evidence; and this is of great importance, as we know from the history of art, that schools may exist which do nothing but propagate bad taste. The taste in French ornamental design is chiefly to be attributed to the adherence of French workmen to models of the most tasteful description.

After the Italian victories of France had filled her palaces with monuments of ancient art, these effected a revolution in her taste, as complete as that which had previously taken place in her government. French art was then clad in an imitation of the garb of that of classic times.

At the last peace, France restored her ill-gotten spoils to the original proprietors; but the French Government perceiving the benefit which had resulted to art in general from the possession of these monuments, determined that, although the originals were lost to France, faithful copies should be preserved; and that the whole country might benefit, and that those who could not visit the capital might have an opportunity of seeing pure models, an establishment was formed in Paris, where, a plentiful stock of moulds being accumulated from the best statues and other sculptures of antiquity, the whole of France has been supplied for years with casts at a comparatively cheap rate. The advantages of such a plan being amply demonstrated by the result, the French continue adding immensely to their collection of moulds, both of statues, bas reliefs, and architectural ornaments. In 1836 they spent in Florence alone, the great sum of 14,000 dollars, equal to L.3111 Sterling or thereabouts.

The artizan, when he wants to aid his invention, can easily procure from the Louvre, at a cheap rate, such classic models as suit his purpose. The advantages to artists of procuring casts of statues, &c., from the very best works of antiquity, are obvious. Schools are rapidly supplied,—private dwellings are decorated with casts,—the constant aspect of such models amongst a people highly civilized, and whose minds are in an active state, must conduce to the dissemination of taste.

The importance of good models to the welfare of art, and to the general diffusion of taste, is a simple truth which does not require so many pages to prove; yet, plain as this truth may appear, no pains have been taken to place such models before the British public: on the contrary, our national collections have been shut against all but the wealthy, by the extravagant fees demanded at the doors, and even in those establishments to which the public have been tardily admitted, the doors are for the most part open only for a portion of the week. The assertion that the promiscuous admission of all classes would lead to the destruction of the objects exhibited has been triumphantly refuted by a happy experience,—witness the conduct of those who visit the British Museum, and, since the fine gallery belonging to the Board of Trustees in Edinburgh has been opened, hundreds of every class have visited it; yet, so careful are the visitors of all ranks, that no injury has been inflicted, and not even an accident has occurred.

The want of proper models has long been felt in England, and often complained of. About the year 1770, his Grace the Duke of Richmond opened a small collection of casts to artists, who eagerly availed themselves of such an opportunity of study. The collection of sculptures in the British Museum has been a prodigious benefit to art, but this benefit is almost entirely confined to London; few students from the provinces can afford to visit the metropolis, and, as a means of generally diffusing taste throughout the country, museums in London are of little avail. Provincial galleries must be opened; meritorious efforts are making in this way, but the difficulty of procuring casts sadly impedes these efforts.

Dr Waagen, in his evidence before the late committee, says, "The best way of forming the taste of the people is by the establishment of accessible collections of the most remarkable monuments of antiquity and of the middle ages. In the capital of the country there should be the chief collection, but it is injurious when all is centralized and confined within the capital; it is also useful, as is partly the case in France, and it is intended to be so in Prussia, to establish subordinate collections in the principal towns in the country. The principles upon which such collections should be formed are the following;—the monuments of the best periods, both of ancient and modern art, which are too extensive and too costly to be possessed by private amateurs, should more especially be placed in a public collection. Collections can only propagate taste and art in a nation, when every man can daily and hourly find free access to the collections of art." Many witnesses before the late committee testify as to the difficulty of procuring good models, and there is but one opinion as to the propriety of establishing local galleries containing fine specimens of art, which would benefit not only the fine arts, but also those which may be properly termed the industrial arts. We may also advert to the difficulties which our artizans find in procuring good models—difficulties which would vanish were an establishment formed where fac-similes could easily be procured of the very objects which have rendered foreign taste superior to our own; and, when we consider that the taste of an individual *is often fixed by first impressions*, we must perceive how necessary it is to fill the country with good models.

When we reflect how little opportunity our artizans have of seeing good models, and how long they have been accustomed to derive their ideas in art from those of a most imperfect description, we cannot wonder at their deficiency in taste. At present our artizans are almost entirely obliged to have recourse to prints when they wish to design. Prints give a very imperfect idea of the graces of execution, and are besides so costly that few designers can afford to purchase them; designs are made up from scraps, and thus absurd mistakes are often committed, notions of unity of style and propriety of ornament being imperfect, incongruous mixtures are made and ornaments misapplied. By the importation of casts of entire designs, the artizan would have an opportunity of studying the free artist-like execution of such models, and he would also learn to appreciate and understand propriety of design.

When we look back to times not long past, we find that the interior decorations of dwellings were then much richer than they now are; we find roofs, halls, staircases, &c., ornamented with stucco-work, and traces of good carving and ornamental painting. These works were chiefly executed by foreigners, who had learned these arts in their native countries. Foreigners are now seldom employed in this country for such purposes, and as we have no artizans who could execute the designs of architects were they to overstep certain limits, our mansions are mostly finished in a plain style; and when attempts are made at a superior style of ornament, the expense becomes enormous—such is the difficulty of procuring workmen. The cure for this, as in all branches of commerce, is to create competition, and this will be effected by the education of workmen in schools; and that their tastes may flow in a proper channel, the purest models must be set before them.

Enough, perhaps, has been said to prove the necessity of the introduction of good models, and a government which wishes to encourage art and ameliorate taste, cannot do so better than by such means; all experience gathered from the history of art proving, that where the best models have existed, schools of the highest character have been formed: and we may anticipate that, by the introduction of like models into this country, and their being placed in situations accessible to all, taste and art of every description will be purified and improved, and a necessary consequence must be, the extensive and well-directed patronage of art by a wealthy and tasteful people.

It is not necessary to dwell longer upon the great benefits which must ensue to our native art from filling the country with the finest models of the art of the best ages:—the proposition is self-evident. Undertaken by government on a liberal scale, the project will certainly succeed, as by opening in London and the other great cities in Great Britain, extensive exhibitions of statues, bas-reliefs, and ornamental casts of every description, there cannot be a doubt that all who require such models will study with eagerness, and that when they can do so they will purchase the casts with avidity.

The advantages to general education will also be great; instead of the imperfect, and often absurd, ideas of the mythology and arts of the ancients conveyed to our youth by the prints in the books they study, the facility with which statues of the divinities of the ancients, bas-reliefs, their habits and customs, costumes and arms, and authenticated busts of all the great characters of antiquity, may be procured, will unquestionably greatly benefit our classic and other students, ameliorate their tastes by habituating them to the constant observation of beautiful forms, impress them at an early age with love and respect for art, and give them a tenfold interest in their studies. A series of historical busts should be in every college; and the unequalled collection in the Capitoline Museum would furnish casts of several hundreds of the most distinguished characters of antiquity.

Thus the highly gifted though poor student would have an opportunity of seeing and studying fac-similes of those foreign monuments of ancient art, which the wealthy alone can travel so far to behold; and to the advancement of art throughout the kingdom and the improvement of our native school, would be added a general increase in classical knowledge and correct ideas of the ancients, only to be derived from such monuments;—and thus taste, science, literature, and art, would be equally benefited and extended throughout our happy country.

Survey of the Surface of the Moon. By WM. BEER and Dr J. H. Mädler of Berlin.*

1. *Physiognomy of the Moon's Surface.* 2. *Supposititious Architectural Remains in the Moon.* 3. *Do Rivers occur in the Moon?* 4. *Lunar Atmosphere.* 5. *Concerning some observations which appear to indicate the existence of a Lunar Atmosphere.* 6. *Non-existence in the Moon of Clouds, Seas, &c.* 7. *Light and Colour of the Moon.* 8. *Physical Remarks upon the Eclipses of the Moon and of the Sun, relating principally to the appearances which the former luminary presents during these interesting phenomena.* 9. *On the effect of the Earth's light upon the Moon.* 10. *On the Meteorological influence of the Moon.*

1. *Physiognomy of the Moon's Surface.*—Even with the naked eye we may distinguish upon the surface of the moon, especially when full, many grey markings of different sizes, some of which are very conspicuously distinguished from the more brilliant parts, whilst others gradually merge into them. The name of *Seas* was conferred on these markings by Hévélius,

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* The above article contains a condensed view of some of the leading sections of the great work lately published at Berlin, under the title of "Der Mond nach seinen kosmischen und individuellen verhältnissen Od. allgem. vergleichende Selenographie. Nebst 5 lith. Taf. gr. 4. Von Wm. Beer and Dr J. H. Mädler." Bibliotheque Universelle, February and March 1838.

without his intending, however, to identify them with terrestrial seas, but only because no better term of comparison occurred to him. Subsequent observations have more and more clearly demonstrated that these portions of the moon could not be entirely covered by a liquid; for they present inequalities of different kinds, also a colour which is any thing but uniform, and besides void hollows are seen, as their shadows demonstrate. Hence, although we ought not from these to conclude in an absolute manner that there is neither water nor other analogous fluid on the moon's surface, nothing more is to be understood by the word *seas* than certain extended and greyish portions which appear somewhat lower, and comparatively more uniform than the brighter parts by which they are surrounded.

On that hemisphere of the moon which is turned towards the earth, there are only two seas, of moderate size, which are isolated and bounded on all sides, viz. the *Mare Crisium*, and the *Mare Humorum*. The larger ones, like the *Mare Serenitatis*, are only partly defined: for not only are they connected among themselves, as are the great oceans of our planet, but they are often likewise devoid of any decided line of demarkation, distinguishing them from the brighter regions. This is particularly the case with the sea *Mare Nubium*, although very near the centre of the moon, and with the *Oceanus procellarum*, which is the largest within our view. At the same time, a slight difference in lustre may be observed, between the contiguous parts of these different surfaces when viewed as a whole.

It may be stated in general terms, that the grey portions occupy nearly two-fifths of the visible surface of the moon. They exist to the greatest extent in its eastern and northern portions, and quite disappear in the most southerly part. Hévélius, as already stated, gave them the names of terrestrial seas; but Riccioli shortly afterwards introduced another nomenclature, which was founded upon the alleged influences of the moon upon terrestrial meteorological phenomena, and upon man's corporeal and mental condition; and this system, notwithstanding its absurdity, has been adopted. These names are now consecrated by the usage of more than two centuries, and they could not be altered without introducing much confusion.

We may admit it as probable, that there are regions on that hemisphere of the moon which is concealed from us, analogous to those to which we have been adverting: for, first, a part of those which we see, advance to the very edge; and again, there are two portions of this appearance, the one of which has been named *Kaestner* by Schroeter, and to the other of which, larger and lying to the north-west, we have given the name of *Humboldt*, which only commence on the side of the moon which is turned toward us, and whose further limits cannot be discovered, even at the epoch of the libration, of all others the most favourable for its perception.

The bright parts of the moon are almost always mountainous, and these mountains surpass the most elevated of the earth, if not in absolute elevation, at least in steepness. Sometimes, though more rarely than on our planet, we perceive simple ranges of isolated summits and small projections; but far more frequently we discover vast masses heaped together, widely extended, and cut by deep transverse valleys. Occasionally, too, we observe rising up, pretty extensive portions of surface, forming a plateau, whence proceed many mountains of different shapes, and at the side of which there exists a very high eminence, whose lofty escarpments descend perpendicularly even to the plain. The Apennine, which is the most remarkable mountain of this last description *(Randgebirge)* has its summit as high as 18,000 feet; and it would appear from the treatise of Plutarch, *De facie in Orbe Lunæ*, that it had been observed by the ancients. Towards the conclusion of the first quarter of the moon, it forms so marked a protuberance on the dark part of the disk, that it might possibly be distinguished by the naked eye. Besides these more common appearances, there are also observed upon the moon's surface lower mountain ranges, and hilly regions. Finally, there likewise exist, and in greater number than on the earth, mountains which are quite isolated, and which are endowed with every variety of form and dimension. Some of these exhibit themselves as ranges without any determinate connection; and sometimes they form a regular circular zone round an enclosed space, which space is on all sides connected with the exterior by lateral valleys.

These circles of the mountains (Bergkränze) lead us to very remarkable forms, which, both by their number and size, and also by the very singular aspect which they present, never fail to produce astonishment in those who examine them with a powerful glass: We allude to the Lunar *Craters.* Their general type is the following. An elevated circular rampart, which, externally is nearly perpendicular, and internally is concave, surrounds a spheroidal hollow, which is usually below the level of the plane surface in its neighbourhood. Sometimes, in the interior of this hollow, mountains rise up which, notwithstanding their escarpment and their very considerable relative elevation, do not at their summits attain the elevation of the rampart which surrounds them, and have no connection with it.

But this fundamental character is so variously modified, and these modifications are so differently associated with each other, and with the diverse forms of the mountains and the seas, that our nomenclature appears meagre indeed when compared with the varied richness of nature, although our distance from our satellite prevents us from recognising many other differences.* We can subdivide these forms according to their relative dimensions, which reach from a diameter of thirty German miles (of 15 to a degree), to the minimum space we can recognise upon the moon with our present instruments, that is about 1500 feet.

The spaces surrounded by mountains of this kind which have the largest dimensions (*Wallebene*) rarely present a simple enclosure, but more frequently a circular congeries of mountains, with projections which are sometimes exceedingly elevated externally. Their inner surface is sometimes plane, sometimes convex, and most frequently studded with mountains and small chains of hills. Their outline usually deviates from the circular form, and sometimes exhibits something like great gates or lateral openings. It is in the southern portion of the moon that these particular appearances are most frequently seen, and sometimes they appear so closely crowded upon each other that the circular form is forcibly transformed into a polygonal one.

The annular mountains, properly so called (*Ringgebirge*) have a diameter which varies from 10 or 12 to 2 or 3 miles. Their form is nearly circular. They exhibit a conspicuous margin, which usually determines their outline. They are met with in nearly all possible situations. They very often exhibit a central mountain in their interior, which is rarely connected with the principal and outer envelope. The internal surface enclosed by some of these circular mountains is upon the same level with the exterior; and the encircling envelope presents something like gates of communication from the exterior parts to the interior. Frequently two of these mountains may be observed very near to each other, and very similar in form and size. This fact is of importance, as it indicates a common origin, whether we regard their nature or the epoch of their formation.

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* It will be remembered the miles in this article, when not otherwise stated, are German; a German mile = about 4.6 English.

In these annular mountains the elevation of the exterior envelope has generally a direct relation and correspondence with the interior hollow. The external slope ranges between a third and a half of the extent of the internal one; and this has led Schroeter to imagine that were the envelope again levelled, it would precisely fill up the vacuity. Were this demonstrated, it would explain how the equilibrium between the different parts of the moon is still maintained, notwithstanding the very great differences in their altitude, and this would also go to prove that it is to the agency of volcanic eruptions that we must attribute the appearances which are met with on the surface. Schroeter has modelled some annular mountains; of these he has compared the projections with the excavations, and has often found a great similarity in the volume, though sometimes there have been sensible differences. However, it is assuredly a very difficult matter in this point to reach any thing like certainty, more especially when we consider the obstacles presented by a distance of 50,000 *German miles* in exactly appreciating all the physical dimensions of an annular mountain.

No kind of proportion exists between the altitude and the diameter of these mountains, and it would rather appear that the smaller have the greatest absolute depths in their interior.

These annular mountains and the analogous formations are found throughout every region of the moon's surface; but they are more numerous and more closely connected with each other in its southern than in its northern parts. In this latter region we should say that they occupied about one-eighth of the surface, whilst in the other they occupy one-fourth. There are certain districts, as for example one situated round the large spot called *Tycho*, where the surface is so studded with these kinds of appearances that it is impossible to find a level which may serve as a base by which to measure the neighbouring heights.

Innumerable circular hollows of smaller dimensions occur upon the moon's surface. These may be distinguished into *craters* and *pits*; confining this latter term to those small cavities connected with which we do not perceive an elevated zone, either because none really exist, or because the size is so inconsiderable that it cannot be recognised. In the craters, central mountains are perceived, and by an attentive examination often a smaller crater may be found placed within the large one. Sometimes, again, two are found so closely approximated that a part of their boundaries is common to both, and it also sometimes happens that, at the point of contact, the zone is interrupted, and there is a passage which unites the two interiors. There are circles of mountains which are in part margins of craters; and, lastly, we may observe that very small craters may be distinguished on the moon's surface, even on those portions which are most uniform.

When the craters are situated at a considerable distance from the limits of the light, and when they are not very steep, their depth cannot be very accurately ascertained. It follows, that, when under a direct light, many of them present luminous and bright spots; whilst on the other hand a certain number of great annular mountains are recognised with great difficulty, and some not at all. Small craters are also seen with much difficulty near the margin of the moon; their rim can then be seen only in profile; their far side is hid as well as the interior, and it is often very difficult to determine whether we are examining a mountain or a crater. It sometimes even happens, that in regions much nearer the centre, a crater exists where we had supposed there was a mountain, and a mountain where we had concluded there was a crater.

It hence results, that it is only by confining ourselves to the examination of regions which are not farther removed from the middle of the moon than 45° or 50°, that we can satisfactorily decide the very difficult question, whether the objects which this luminary presents are subject to true *physical* changes, which we have the power of recognising, a question regarding which, in our opinion, nothing at all is known at the present time. The circle above referred to, may be extended some additional eight or ten degrees, by taking the libration into account, and profiting by it properly. Within these limits an experienced, attentive, and persevering observer, especially if free from preconceived bias, and if supplied with powerful instruments, which clearly define the objects, may be preserved from optical illusions. Whatever is seen beyond the limits we have just mentioned, may be very useful in determining the general relations of the different parts of the moon; but the region near the margin is, for a variety of reasons, unsuitable for any conclusions which are to be founded upon the accurate appreciation of minute details.

Those mountains which are situated in the interior of the different circular zones which we have been describing, vary considerably. Sometimes they are solitary, and at other times they are conglomerated in masses, and more rarely in mountain chains. Their summits never attain the elevation of the zone, and sometimes not even that of the surrounding surface. The shadow of the zone usually covers them a long time before the sun sets on that locality in which they are situated; and it is not a little curious to witness them reappear, as very minute luminous points, in the midst of the deepest shade which envelopes them.

From what has now been said, it will appear that the forms of the craters of which we have been speaking, have very little external conformity with analogous appearances in our planet. The largest craters of our volcanos can scarcely be compared with the smallest pits on the surface of the moon. We can always perceive the ground at the bottom of the lunar craters; whilst many of those of the earth are real abysses. The Laacher lake, near Andernach, the mountain Albano near Rome, and other localities which might be mentioned, exhibit but the feeblest and most obscure type of these lunar craters; and these formations are moreover confined in this planet to some very special regions, ilst they decidedly predominate over the moon's surface. On the other hand, the broad and long valleys of our mountain chains, as of the Andes, the Alps, and the Himalaya, are quite unlike any thing found in the moon, or the analogy at all events is a most obscure one. Nearly all the forms of the earth's mountains are changed by the action of water and atmospherical variations, whilst these modifying agencies are probably wholly wanting in the moon.

We have still to make a few observations upon an appearance in the moon, which presents a distant analogy to our rivers, viz. the grooves and furrows (Rille) which are so remarkable and at the same time so difficult to recognise. They are long and narrow hollows, sometimes straight or slightly arched, and sometimes sinuous or crooked, which have been hitherto discovered only in a small number of localities in the moon. Sometimes they extend from one small crater to another, and in other cases, they appear to be isolated in the grey plains, terminating in a manner in no way peculiar. Often they are stopped by the mountains which they do not traverse. The grooves which are situated near Higinus and Ariadeus, as well as that large one which is found near Aristarchus, are those which may most easily be discovered; and in them we perceive, notwithstanding their narrowness, a distinct trace of shadow. The numerous grooves situated near Triesnecker, and the small ones in the region of Posidonius are discovered with more difficulty. Their whole number, as now established, is still limited; although we have discovered a certain number in regions where former observers never noticed them. Thus we have found four in the south-west angle of the *Mare humorum*, one of which runs towards Hippalus, and abuts to the north in the region of Agatharchides. The district near Triesnecker is still richer in fissures which partially unite and again separate as in veins, and thus form a particular system. Some of them are from two to three miles in length, the majority are from ten to fifteen, and a small number extend from twenty-five to thirty miles. We have found none that exceed this maximum.

The question now occurs, Are these grooves lunar rivers? to which we answer, that their resemblance to rivers is a very slight one. They do not descend from the mountains, but rather cross their hollows. Those which are situated upon the middle of the moon, especially the one near Higinus, shines brilliantly when the sun is up. This, however, is not to be ascribed to the effect of the reflection from a liquid surface as from a mirror, but is owing rather to a great elevation or escarpment of the internal bounding wall. If these grooves had any thing in common with our rivers, the subordinate character of these appearances evidently proves that the existence of water in the moon cannot be compared with what we find on the Earth. Were this a system of rivers, they would be arranged altogether differently. They might, indeed, be more or less compared to rivers, did such exist, on the steppes of Persia or of Arabia, or on the northern border of the parched Sahara.

Still less can it be admitted that these grooves are great artificial roads. It would be to allow by much too great a similitude between terrestrial and lunar relations, to presume that the inhabitants of the moon engage in such works as these. The fact that we can perceive them, which supposes a breadth of at least twelve or eighteen hundred feet (and the majority are much wider), is in no degree favourable to the supposition. This is equally true when we consider the manner in which they present themselves to our examination. In the region of Güttenberg there are three long grooves which are quite parallel, and distant only two miles from each other; and analogous phenomena are to be detected in other places. No more are they directed towards remarkable points, but generally terminate in an open country.

2. *Supposititious Architectural Remains in the Moon.*—We may here observe that the zeal which has been displayed in seeking for the traces of lunar architecture, has not hitherto led to any successful result, nor, in fact, to any prospect of one. It is exceedingly improbable that, even in future ages, and making every allowance for the advancing improvement of our mechanical and optical apparatus, there shall ever be discovered in the moon, objects analogous to our cities, roads, or ramparts. All that civilization has hitherto produced, or is now effecting on the surface of the Earth, is, independently of man's intellectual powers, effected by two principal agents, one of which depends upon the changes of the atmosphere, and the other, upon the proportions which subsist between mechanical powers and

the masses on which they are brought to act. The former determines the occasions and the principal directions of our corporeal activity; and the latter assists us in appreciating what it is possible for us to accomplish in all that relates either to quantity or quality. In other words, man builds mansions, because those which nature provides for his accommodation appear insufficient in the atmospherical circumstances in which he is placed; with much labour he makes regular roads, because, according to the nature of gravity at the surface of the Earth, his own transport, and that of goods, would be exceedingly difficult without artificial routes. We oppose barriers, to the injuries of time, whose form and magnitude are determined as much by the object we have in view, as by the forces we have at our disposal. Our philosophy having not as yet succeeded in procuring for us a general and perpetual peace, we have built fortifications which, previous to the discovery of gunpowder, were very different from what they now are, and which will be very different again, when steam has come into somewhat more general use. A number of our arrangements have relation to the variations of the seasons, so remarkable in our planet. Thus, in fact, every thing which man effects upon the Earth is associated with the special circumstances of our globe; and when what is requisite for us is naturally prepared, there is no necessity for our substituting our individual exertions. If the shores of the ocean afford sufficiently good harbours, we never think of establishing them; if rivers answer the demands of commerce, a canal is not made; and if nature's precipices would afford sufficient shelter, we should be spared all the trouble of walls and enclosures.

What right, then, have we to expect artificial productions which shall have an analogy, even the most distant, with terrestrial objects in a heavenly luminary where the existence of an atmosphere is, to say the least, exceedingly doubtful, and where there are neither winds nor rain; where of water in a liquid state there is none, and where the gravitation of bodies, and, consequently, the resistance of matter, is six times less than upon the Earth; without even alluding to the immense differences the earth presents in relation to its days, its seasons, its temperatures, &c. Those astronomers who have given themselves with so much keenness to conjectures, might have saved us from many useless hypotheses, which are any thing but creditable to science, if they had attempted to base their ingenious conjectures upon the evident data of theory and observation, rather than on sheer possibilities. It is hoped that our powerful glasses will enable us to dispel the obscurities which surround the moon. But instead of this these individuals only deepen the obscurity; and never will it be removed so long as the reins are in this way given to mere conjecture. Accurate observations alone, freed from all preconceived opinions, and repeated with indefatigable perseverance, are the sole means in our power by which progress can be made.

3. *Do Rivers occur in the Moon?*—The question of the existence of a system of drainage by means of rivers in the moon, is intimately connected with the other which refers to the nature of the large grey markings which it presents. If these latter be not really seas, it is quite useless for us to search on the moon's surface for rivers analogous to those we find on the Earth. Now, it is to be observed that in all the regions to which the appellation of seas has been given, besides the annular mountains, and the clear and brilliant peaks which have sometimes been taken for islands, there are likewise many flat and broad crests, which extend themselves in all directions, and which, as to colour, in no respect differ from the lower districts, as should happen were these flat elevations land, and the others water. Towards the epoch of the moon's quadratures, a great number of those surfaces which are among the most brilliant at full moon, assume a darker shade, which is altogether analogous to that of the sea, although there can be no dispute that they partake of the nature of continents. Nay, there are even mountainous regions, such as that of the neighbourhood of the spot *Schroeter*, which appear at the full moon darker even than the seas themselves, although it is evident, at the first glance, when they exhibit themselves near the margin of the luminous portion, that it is quite impossible they could be covered with water. It follows, that we cannot conclude from the dark colour of any region, that it is liquid.

4. *Lunar Atmosphere.*—These considerations have an intimate connection with the question of the existence of a lunar

atmosphere. It has been attempted to establish its existence both by observations, and by arguments of an entirely different nature. Schroeter imagined that he perceived traces of twilight in that part of the moon which was dark, and principally along the obscure border, which may be seen when the lunar crescent is reduced to a small luminous thread. According to his observations, he has calculated what the thickness of the moon's atmosphere should be, and found it amounted to a 28th part of the Earth's atmosphere. Melanderhjelm has endeavoured to demonstrate, theoretically, that the thickness of the atmospheres of two celestial bodies should be proportional to the square of the power of gravity at the surface of these bodies, which would give to the lunar atmosphere $\frac{1}{36}$th part of that of the Earth; but his conclusion should be limited by this condition, viz. the universal existence of atmospheres.

M. Bessel has demonstrated (Astr. Nachr., No. 263) that after attributing the greatest possible altitude to the mountains behind which stars in a state of occultation disappear, say 24,000 Fr. feet, as also allowing the greatest possible effect which can arise from refraction, viz. a difference of 2″ between the diameter of the moon according to direct measurement, and its extent deduced from the duration of the occultation; and by still further admitting that the temperature was at 32° Fahr. from the surface of the moon to an altitude of 24,000 Fr. feet: still with all these concessions, of all others the most favourable for the extent of the lunar atmosphere, yet its thickness would amount to only $\frac{1}{968}$th part of that of our atmosphere, supposing that the two were of the same nature. Upon the supposition that the temperature and the composition of the atmosphere are different, still the results are analogous. Thus, supposing it was oxygen gas, the greatest thickness would only be $\frac{1}{863}$d part, and it would be $\frac{1}{500}$th part with a temperature of about minus 240° R. If it is moreover considered that the stars, on reaching the obscure edge of the moon, always disappear suddenly, and without the slightest diminution of their brilliancy, the only conclusion which remains, is, that the moon possesses no atmosphere at all comparable with ours.

Concerning the alleged twilight, observed by Schroeter, this feeble and obscure phenomenon, which we ourselves have never succeeded in distinguishing from the effect of the earth's light upon the moon, might easily, if it were more than a simple optical illusion, be explained in many other ways than by having recourse to a lunar atmosphere. The sun's diameter alone must occasion a small twilight with a mean breadth on the moon of 2,29 miles, and geocentrically must subtend an angle of 9″; and which may, towards the margin, when the moon shews only a very small crescent, extend to many minutes. The inequalities of the surface, too, must occasion a somewhat more extended twilight; the sides of illuminated mountains must reflect a part of their light upon the valleys below, and we may, perhaps, thus explain the phenomenon observed by Schroeter in the moon's southern horn, and which has hardly been noticed in any other point. The mountain Doerfel,* which is probably the most elevated in the moon, thus projects its light over a widely extended space on the two halves of the moon's surface; these points may be considered as isolated islands of light, which rise conspicuously above the extremity of the crescent, and in this locality affect both the phases and the libration.

The equal distinctness with which all parts of the moon are perceived, those near its edges as much as those in the centre, is another positive reason against the existence of any medium of imperfect transparency on its surface: for all the outlines in the different regions, when observed at the same time, and under equivalent circumstances as to light, are distinguished alike distinctly; and towards the edges they do not present other difficulties in their representation and delineation, than those arising from their being fore-shortened. Attentive observations of the spots of Mars and Jupiter, have led us to recognise, that, towards the edges of these planets, every object becomes indeterminate, and disappears; we have ourselves experienced this, and in all probability it arises from the atmosphere of these planets enfeebling their light.* But, as we perceive nothing of this sort upon a luminary which is a thousand times nearer us, the cause which produces it may be considered as absolutely wanting in the moon.

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* This mountain is among the number of those which are situated towards the limit of the visible part of the moon, and whose height cannot be determined by the length of their shadows, but only by the approximative method of the projection which their profile forms upon the margin of the disk. Two measurements of this kind gave MM. de Beer and Mädler 3800 toises as its height.

5. *Concerning some observations which appear to indicate the existence of a Lunar Atmosphere.*—We have already seen that it may be regarded as demonstrated, that the moon has no atmosphere which, in any respect, is comparable to that of the earth. It does not, however, necessarily follow, that it is completely devoid of one; and some appearances have been witnessed which may be referable to a cause of this kind. Such, for example, are the changes in brightness, as well as in colour and form, which have been observed in some stars a short time before their occultation behind the moon. Some astronomers, it is true, have never recognised such appearances. But others, we must admit, equally worthy of confidence, have verified them. Messrs Beer and Mädler mention, among others, that two observations have been communicated to them by M. Boguslawski of Breslau, in which two small stars assumed, for a minute or two before their occultation, a form which was elongated in a direction perpendicular to the margin of the moon, and again, just before they were hid, appeared as luminous points. Our authors themselves, on the 25th of April 1836, observed η of the lion, an instant before its occultation behind the moon, become somewhat redder, and slightly diminish in lustre. They thought also that they perceived, on many occasions, when an annular mountain began to be illuminated, and when it was still darkness all around it, that slight changes of colour were manifest, consisting in a bluish light, which rapidly transformed itself first into a pale yellow, and then into a more decided yellow.

If an atmosphere exists round the moon, corresponding to what the small size of the satellite might induce us to expect, it is also possible that local causes might occasionally disturb and obscure it, especially during the night; and that would explain why the appearances to which we have just been alluding are seen only near the time when the sun rises upon a particular region of the moon, or towards its obscured margin.

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* The most considerable of the two spots which we observed upon the disk of Jupiter, from November 1834 to April 1835, continued visible no longer, after passing the central point, than during one hour and twenty-four minutes and one hour and twenty-seven minutes, on two opportunities which were peculiarly favourable; in all other instances it disappeared sooner. It has not been seen, therefore, farther than at 54° of the point of conjunction; so that all that is required to render it invisible is an obscuring thickness double of that which exists on the middle of the disk.—(*Note by the Authors.*)

As yet we can in no way value, even by approximation, the thickness or density of this covering. It is probably much smaller even than the maximum arrived at by Bessel, viz. $\frac{1}{988}$th part of the density of our atmosphere, and it is too feeble to be able to produce, in ordinary cases, the effects of refracting or obscuring objects.

So far as the different classes of celestial bodies, viz. suns, planets, satellites, and comets appear essentially to differ in their various relations, and to have, in common, scarcely any thing more than what is a necessary consequence of the law of universal gravitation, we may also regard it as probable that the enveloping gases of these bodies are different, not only in their relative quantities, but still more in their constitution and in their chemical relations.

6. *Non-existence in the Moon of Clouds, Seas, &c.*—The non-existence of a lunar atmosphere must sweep away all hypotheses concerning clouds, smoke, nebulosities, rain, snow, &c. occasioned by the presence of water, as well as the existence of water itself. From this circumstance it follows, that there is a complete and total difference between the surfaces of the earth and the moon, in relation to the whole economy of organized nature. The moon, then, is in no respect a copy, and far less a colony, of this earth. It is impossible to compare with each other the planetary and lunar vital forces; and the ulterior discussion, whether the moon is inhabited by men, must appear absolutely superfluous.*

No more can there be any seas in the moon. The hemispheres, both visible and invisible, must be continents throughout all their extent. Nor is there the slightest ground for believing there is any system of rivers at all analogous to ours.

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* MM. Beer and Mädler have remarked in the preface of their work, that, as the distance of a German mile is the farthest that the most piercing sight can distinguish a man, or the largest of terrestrial animals, without the aid of glasses, and, as our distance from the moon amounts to 51,000 of these miles,

The forms of the lunar mountains perfectly agree with this conclusion. Convex slopes (convexe böschungsformen), and escarpments, far more remarkable than those of the earth, predominate in the moon. Slopes at an angle of 45° and even more, are there the most common; and not only the bright parts of the surface, but also the seas, are covered with great rocky masses. As the density of the material of which the moon is formed is scarcely above the half of that of the earth (0.57), we cannot easily admit that a greater number of compact masses of rocks can be found on the moon than on the earth; and these masses could not be in a state habitually to resist the action of water and of the atmosphere, and to maintain themselves in a position so steep and perpendicular. Great annular mountains are seen, with a regularity of form both on their internal and external surface, which they could never preserve on the earth, exposed, as they would be, to the influences of air and water; and this shews that they have been exposed to no such influences, since the time of their formation. Valleys, except, of course, craters, are comparatively exceedingly rare in the moon; and all their relations are perfectly different from those we find upon our globe; in fact, there are no broad and long ones, through which rivers might flow. The formation of regions in successive terraces is also wholly wanting in the moon. The declivity of the high mountains is as rapid as that of a wall, and does not terminate in a valley, but

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we must succeed in obtaining instruments which will magnify, in linear size, 51,000 times, before we can hope to discover objects of that magnitude in the moon. But, up to the present time, they add, the magnifying to 300 times is the utmost extent we have succeeded in making any observations upon the moon, whose results can be compared with each other. There is required, therefore, a farther improvement upon our astronomical instruments, which shall be to our present in the ratio of 510 to 3, or of 170 to 1. It is also necessary to suppose that the terrestrial atmosphere should become 170 times more transparent; and that the inconvenience arising from the apparent diurnal movement of the moon rendered 170 times more rapid, should be removed, before we can ever hope to perceive from the earth, the living beings who may possibly inhabit that heavenly body, which, compared with others, is situated in our own immediate vicinity.

only in the lowest portion of the surface of the ground. Nor is the formation of terraces common, except in cavities in the interior of the great mountains. Our satellite thus exhibits its mountains in their original form, an occurrence which is seldom or never witnessed on the earth.

7. *Light and Colour of the Moon.*—That we may complete this general description of the moon's surface, it is necessary to point out the variety of colouring which may be detected. The difference betweeen the bright and the dark parts may be detected by the naked eye, particularly at full moon: and this circumstance proves, that, so far as these appearances are concerned, shadows are not at all the cause, and that differences in the level have no share in these differences of shade. All the shadows which shew themselves on the moon at the epoch of her phases, are always perfectly black, whilst the darker portions of her surface, such as Grimaldi, Plato, Boscovich, and the small spots in Petavius, William Humboldt, and Alphonso, are always grey only; so that the finest points of shadow which are projected over these surfaces can be measured as accurately as in the brighter districts. In the moon, therefore, there are differences as to the quantity of the solar light which is reflected: and as we see very distinct traces of such a diversity in the obscure portion of the moon, this same phenomenon occurs likewise in the earth's light, and in all other kinds of light.

It is proper, therefore, that a scale of these gradations of light should be established, and such an one as the eye, assisted with good instruments, may be able to distinguish its divisions with some degreeof certainty. Preceding selenographers were content with three degrees. Schroeter, and after him M. Lorhmann, have adopted a scale of ten degrees, and we, in this respect, have followed their example. The zero of the scale corresponds to the shadow which is projected by the mountains. The first three degrees may be denominated grey, the fourth and fifth, light grey, the sixth and seventh white, and the last three shining white. The 1st, 9th, and 10th degrees are found only on small parts of some spots. The first is that of Grimaldi and Riccioli; Boscovich, Julius Cæsar, Plato, and a part of Schikard have 1½ degrees of light. Numbers 2 and 3 are the usual degrees of the brightness of the seas; belonging to the darkest of which are the *Mare Crisium*, some contracted parts of the *Mare Tranquillitas*, and the *Mare Nubium*, as well as the margins of the *Mare Serenitatis*. The seas situated in the more elevated northern latitudes constitute the 3d degree, and consequently are distinguished with greater difficulty from the surrounding regions. Besides, we here and there find, between bright parts, small, narrow, and tortuous valleys, whose brightness is at 2°; and the 2d and 3d degrees of light are also very common at the surface of annular mountains, as well as upon the hilly regions. There are also three high mountains in the region of *Pythagoras* which stand at 2½° of the scale, whilst all the surrounding regions, including mountain and valley, have a brightness of 4° and 5°. This fact is altogether an anomalous one; for although there are many hills and mountains in the moon of a sufficiently dark shade, still the whole of the country which surrounds them is likewise of the same tint.

The brighter regions vary between the 4th and 6th degrees. The last of these two, belongs only to a part of the south-west quadrant of the moon, and to parts near its edges. The margins of most of the annular mountains range from 4° to 7° of illumination, and many of them on their external parts present an equal brightness, or very nearly so. The summits of the isolated or concentric mountains have from 6° to 8°, but generally, it is not the most elevated peaks that have this brightness. A few, which are not much elevated, attain to the 9th degree. There is, near *Atlas*, a small system of hills, and to the north of *Lexell*, a region almost uniform, which reach this degree of brightness; but, generally speaking, it is in the craters, and in the annular mountains, that we find the last three degrees of brightness.

There are some craters which shine brightly not only towards their edges, but also throughout their whole internal surface. These are the clearest of all the almost innumerable luminous points, with which the disk of the moon is studded at the full. A circumstance sufficiently remarkable is this:—that at these times we have no criterion by which to decide whether these points are elevations or depressions. We must wait the period of the phases to determine whether the luminous appearance is a crater or a mountain, or if it be neither the one nor the other.

There is only one annular mountain, viz. *Aristarchus*, and one point in *Werner*, which reaches the 10th and highest degree of illumination. When the sun is sufficiently elevated, all the interior surface of *Aristarchus*, as well as the annular mountain itself, shines with a light of sparkling whiteness. The central mountain, and two or three points of more feeble radiance, may also with difficulty be distinguished. Hard by, we find *Proclus* whose zone, and interior very smooth slope, have a brightness of 9°, whilst the interior itself is at 8°. The central mountains, with a very few exceptions, are always brighter than the hollows which they surround.

But numerous as these brilliant craters are, they scarcely form a half of all the forms of the total number of craters. There are many, and amongst others the largest and deepest, which completely disappear at full moon. It is no uncommon occurrence to notice two craters very near each other, and which appear to be equal in diameter, in steepness, and in depth, one of which will shine at full moon with surprising splendour, whilst the other cannot be distinguished but with the greatest difficulty. So soon, however, as the shadows begin to appear, the latter will begin to shine, and the other to be obscured, and when the light is about to leave their neighbourhood they appear quite equal. Moreover, towards the epoch of the phases you can scarcely any longer distinguish any difference of outline in the neighbourhood of the limits of the light ; and the escarpments of mountains, which are turned towards the sun, as well as the plains and more gentle slopes, shine, as if they were illuminated under a greater angle.

Among the most remarkable and most inexplicable appearances in the moon are the systems of rays which it presents. Seven of the largest annular mountains, viz. Tycho, Copernicus, Kepler, Byrge, Anaxagoras, Aristarchus, and Olbers, are surrounded with long and broad pencils or luminous bands (lichtstreifen) arranged in a radiated form. Mayer, Euler, Proclus, Aristillus, Timocharis, and some others exhibit the same appearance on a smaller scale, and in a less striking manner. These bands usually commence at a small distance from the annular zone of the mountain. The extremity nearest this point, which is comparatively obscure, has 4° of brightness (only 2° upon Aristarchus); the band then extends to 30, 50, and sometimes even 100, and 120 miles across the plains, mountain-chains, isolated mountains, craters, fissures, and, in a word, across every appearance which the moon presents, without being in the slightest degree modified by any of them. They are found in such numbers in the neighbourhood of the annular mountain, which constitutes their centre, that they form a kind of aureola or glory (nimbus) round it; it is peculiarly conspicuous round Kepler, and is scarcely visible round Aristarchus. After this they ramify; sometimes they are somewhat curved, but not often; they are connected by transversal bands; they are sometimes, too, somewhat interrupted by portions where the light is more feeble, and sometimes, towards their middle, there is only a dark ray for some extent. Often, instead of this radiated form, we notice two or three, or a greater number of rays which are perfectly parallel. In some instances they terminate suddenly at a crater or annular mountain; they always remain distinctly visible at full moon; many of them approach the moon's edge, and there lose themselves imperceptibly in the brightness of this part: when this does not happen, most of them terminate insensibly in a plain or mountain. The most extended of these radiated systems is that of Tycho. Here more than a hundred very distinct luminous bands, some miles in breadth, project from it in all points, and traverse nearly the whole of the south-west quadrant, and a great part also of the south-east one. Two of these bands extend, though unequally, from thence; the one of them, which is double, having a dark intermediate space, directs its course in a north-easterly direction towards the *Mare Nubium* and the *Oceanus Procellarum*, where it loses itself after a course of about 150 miles. The other, simple and not so brilliant, traverses nearly the whole visible surface of the moon, reaches, still very feeble, to *Menelaus*, suddenly becomes clearer on the dark *Mare Serenitatis*, divides it into two almost equal halves, and probably advances still further towards the north, till it is lost in the northern regions on the margin of the disk; it thus traverses 100° of a large circle of the moon, over an extent of more than 400 miles. When these bands are much extended, they so much surpass in brightness every other part, during full moon, with the exception of the bright craters, that nothing can be determined concerning the forms of the ground or surface.

These luminous bands are not elevations. When low mountain ranges come in their track or neighbourhood, they follow neither their course, nor their outline; and still less do mountains properly so called. The mountains and the bands may rather be said to alternate with each other; for, when the mountains commence distinctly to appear, the bands disappear, and conversely. The bands may be obscurely perceived in some plains (as in Stöfler and Meton) even with an oblique illumination, after they have ceased to be visible in mountainous regions; but this never continues after the sun has set.

It is true that the very distinct band which traverses the *Mare Serenitatis*, is accompanied by low ramifications *(Bergadern)* which are parallel to it, and of which some parts traverse it. But the band itself appears so completely on the level of the uniform surface, that it always disappears in the neighbourhood of the confines of the light. We have verified the disappearance during six hours of observations made in a night which was peculiarly favourable. On approaching the confines of the light, the mountain ranges became more distinct, and generally visible for the first time, while the bands disappeared from our eyes without leaving a trace behind, which would not have happened if they had possessed even one degree of inclination.

Besides the principal radiated systems we have just been considering, which, though they have some minor differences, are nevertheless very similar to each other, we also discover upon the moon's surface many of these appearances, which are incomplete, and sometimes even solitary bands. Thus three rise from Proclus, forming with each other almost equal angles, of about 120°, two of which are obscure and difficult to recognise, while the third is more distinct, and terminates in a marked bifurcation. On the *Mare Fecunditatis*, we perceive another appearance of this kind, which has precisely the appearance of a comet with a double tail. The nucleus is represented by two neighbouring craters of equal size; the rays are linear, equal in size, breadth, and clearness; for a certain distance they augment in capacity, then diminish in intensity, and insensibly fade away. There are also in the *Mare Nubium*, and in the northern part of the *Oceanus Procellarum*, many craters of a mile, and a mile and a half in diameter, surrounded by a brilliant space, which is not subdivided into rays, but which is partly concentric and partly eccentric; this space extends only from four to six miles, and insensibly loses itself in the darker surface which surrounds it. Finally, we may also perceive in these same seas, pale rays running in different directions, which do not exhibit any distinct mutual dependence, or any common point of origin, but some of which present, towards the time of the change, the appearance of small mountainous ramifications, though the majority are still independent; for, by a somewhat more direct illumination, they present precisely, or very nearly so, the same tint of colour as the surrounding surface, and disappear from our view.

In the preceding remarks we have considered only the different degrees of illumination which the surface of the moon presents; but it should likewise be observed that we may also perceive under favourable circumstances, differences in colour which are really specific. All the *Mare Serenitatis*, with the exception of the marginal portion, which is darkish, has a shade of beautiful green. In the *Mare Crisium*, the green is mixed with a dark grey. These two colours are separated in the *Mare Humorum*, the green occupying the greater part of its surface. The sea called the Cold Sea presents a pale greenish-yellow, which is uniform throughout. The enigmatical colour of what is termed the Marsh of Sleep, which is a region of well-defined hills, appears to be yellow, and exhibits a reddish lustre in a few places. The dark grey appears to exhibit in many places differences which are somewhat analogous to those just stated. Whilst in certain regions it appears to be a mixture of white and black, many others have all the appearance of an entirely uniform steel-grey tint. As the different shades of light only manifest themselves distinctly at the time of full moon, so it is the same in a still greater degree with respect to the colours which are specifically different, which the most careful investigation will fail in discovering two or three days either before or after the luminary's opposition to the sun, and which, in fact, cannot even then be perceived, except under very favourable circumstances. In truth, we have doubts whether all eyes and all telescopes have the capacity of perceiving them; for, although their several limits are sufficiently marked, the differences themselves are very small.

The great luminous bands also mentioned above, likewise sometimes present a diversity of colour, when compared with the brighter regions which surround them. They are sometimes distinctly seen to arise in certain parts of the surface which are as clear and brilliant as themselves, and an attentive examination makes it apparent that the bands are milk-white, and the surrounding region is yellowish-white. It is true that we have as yet determined this slight difference with certainty, only in a few instances, particularly in those bands which appear to issue from Tycho towards the southern border. It would be difficult to admit that the shining white of Aristarchus is any thing else than a very intense yellowish-white.

It is, we apprehend, quite in vain to search for the cause of these appearances. As to the different degrees of brightness, it is evident that the different nature of the surface must, upon the moon, as upon our globe, occasion various degrees of reflection. We see, in truth, that upon our planet, independent of the differences of land and sea, the aspect of different portions of the country when viewed from a distance is very different, and is probably modified by the seasons. Respecting the shining brightness of a great number of craters, it may be explained by admitting, that through the influence of their large concavity, they act as a mirror, and reflect a concentrated light towards us. A confirmation of this explanation is to be found in the fact that it is the eastern side which shines most before full moon, and the western side after it; and that the libration also appears to have an analogous effect. Perhaps Aristarchus is nothing else than the most perfect of all the reflecting surfaces, whence it enables us to perceive an image of the sun itself, or at least of a part of that luminary. At the same time the phenomena are much too varied to be satisfactorily explained by this single circumstance alone, and there are among other objects, mountains and portions of plains, which equal in splendour the most brilliant craters. As to the real nature of the moon, with the exception of some negative determinations, we know nothing special or exact. We may at least, however, be permitted, after the preceding investigations, to reject, as inadmissible, and contrary to observation, the opinion that the moon is entirely covered with ice or with snow, or is nothing more than a compact mass of rocks.

Regarding the luminous bands, Schroeter and preceding selenographers have regarded them as chains and ranges of mountains, but our investigations expressly contradict this opinion. Herschel believes that they are streams of lava which have issued from the great craters, and have spread themselves in all directions.* Being ignorant of the forces which may be in activity in the interior of the moon, we consequently cannot bring forward the great altitude of the zone of Tycho, Copernicus, &c., nor their other relations as to form and size, as decisive objections against this opinion. But a current of lava ought, more or less, like any other current, to tend towards the hollows when descending from a height, and should follow the sinuosities of the valleys; or, should it be so powerful as to force forward its route in a straight line, and not to be arrested by ramparts of from 6000 to 10,000 feet of vertical elevation, it should at all events fill up the hollows which lie behind these ramparts, instead of simply continuing its progress, and leaving every thing untouched to the right and left. If, for example, we examine the bands which pass towards Stöfler, Kies, Bouillaud, Maginus, and other annular mountains, we shall be easily convinced that, so far as it is a question of gravitation merely, the lava or any other fluid could never follow the courses which we have traced.

Still less can it be thought that they are jets of continuous light proceeding from a central point; for the bands extend over the surface of the moon ten or twenty times beyond the spaces whence the annular mountain may be seen, and it often happens that, when the point has been for long invisible in the obscured portion of the moon, the extremities of the bands may still be distinguished.

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* Herschel, it is true, has no where so expressed himself in so many words; and perhaps he applied his remarks, not so much to the luminous bands, as to the small mountain ramifications, which extend, for example, from Aristillus and Autolycus, to the distance of from eight to twelve miles. (*Note by the Authors.*)

The only conclusion, then, to which we can arrive is, that, by some operation of nature, the interior structure of the lunar soil has experienced, at those places where the bands are found, some change which in them has notably increased their power of reflecting light. As to the nature of this change we can do nothing more than form conjectures, but it seems indubitable that it has had an intimate connection with the formation of the annular mountains, which are found exactly in the central point of these bands.

8. *Physical Remarks upon the Eclipses of the Moon and of the Sun, relating principally to the appearances which the former luminary presents during these interesting phenomena.*—It has long been observed that the moon does not invariably present the same appearance when totally eclipsed. In by far the greater number of instances this luminary may still be distinguished; and in fact it generally exhibits a faint reddish aspect, more or less distinct, which enables us, with the help of glasses, to recognise the most of its spots. This light was remarkably intense during the eclipse of December 26. 1833, the state of the atmosphere being very favourable for the occurrence.

MM. Beer and Mädler discovered, on that occasion, the smallest objects that are usually detected on the moon's surface; as, for example, the hilly groups which are situated between Pliny and the promontory of Acherusia, each part maintaining its relative degree of illumination as in full moon. The margin of the earth's shadow was very distinct, and as a whole it was regularly elliptical. Towards the central part of the shadow the objects were less distinctly traced, and the great seas were seen with difficulty, although the spots were visible. In other total eclipses, again, the limits of the shadow and the aspect of the moon itself have appeared much more indistinct, a dark and vapory glimmering being all that could be detected; and in other cases, as on the 10th of June 1816, the moon was wholly invisible during the great erpart of the total eclipse, although it did not pass towards the centre of the shadow.

It would appear that these striking differences are not owing to the greater or less dimensions of the section of the cone's shadow which traverses the moon, nor to its distance from the centre of this section. According to Messrs B. and M., they depend, to a great extent at least, on the state of the terrestrial atmosphere in the regions where the sun rises and sets at the time of the eclipse, the sun's rays being refracted by the horizon of these regions, so as to be propagated even to the moon, with the various shades of colour belonging to our twilight. As it is easy to calculate in each case what is the terrestrial zone to which the sun's rays are tangents, and in what part of this zone the inflection of these rays will direct them towards the moon, we may determine before hand, in certain cases, and to a certain extent, the general circumstances of an eclipse in these matters; and still more may ascertain afterwards, if there has been an accordance between the circumstances of the terrestrial atmosphere and the appearances which the eclipse presented. It was thus, that, during the eclipse of June 10. 1816, the refracted solar rays which could reach the moon must have traversed the southern portion of the zone of contact (*tangence*), which was then in its winter, and almost entirely oceanic, so that the invisibility of the moon might easily be explained by the probable haziness of this part of the earth's atmosphere.

The feeble visibility, and the subsequent disappearance of a particular region of the moon, observed by Eule in Dresden, during a total eclipse in the year 1818, may be explained by the momentary brightening of some parts of the zone of contact.

In conclusion, Messrs B. and M. remark, " we will not venture to determine, as some individuals have done, whether, at these periods, the moon exhibits a light which belongs to itself, and which endows it in all cases, at the end of a certain time, with a red tint. Hahn admits, that in any lunar region there is always, when it is illuminated with sufficient directness, a kind of phosphorescence which we cannot perceive except during eclipses; and he moreover believes that the surface of the moon receives from the sun a greater quantity of light, and a less quantity of heat than the earth. The latter of these statements is probably true, while the former is by no means a necessary consequence; and we conceive that the explanation above supplied is sufficient to account for all the phenomena of eclipses which have been accurately observed. We have remarked that the red colour appears much sooner when we remove from the field of vision that portion of the moon's disk which is already bright. As by this step we only diminish the enfeebling optical effect of this portion on the other, without entirely destroying it, it is most probable that we should speedily remark the red colour, could this optical influence be completely removed, even should the vivacity of the colour be subsequently increased.

It is likewise to the influence of the terrestrial atmosphere that Messrs B. and M. principally attribute the circumstance, that, in eclipses of the moon, the pure shadow is somewhat more extended than it should be, according to the dimensions of the globe. Their observations of the entrance of many lunar spots into the shadow during the eclipse of the year 1833, and of their exit from the shadow, have yielded them, for the half of the eclipse, a mean duration, greater than it should have been, of from ninety-three to nine-five seconds of time, and a corresponding augmentation of the semi-diameter of the shadow of about a fiftieth part. In the partial eclipse of the 10th of June 1835, they found about a twenty-eighth part was the augmentation of the semi-diameter of the shadow, resulting from the comparison of their observations with the calculations. Lambert estimated this increase at one-fortieth, and Meyer at one-sixtieth. Messrs B. and M. also conceive, that this apparent augmentation might partly be owing to the smallness of the solar crescent which is seen upon the moon during eclipses, and which, in certain cases, may cause the mixed, or imperfect shadow, to be confounded with the pure one.*

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* Dr Mädler has published (in *Astr. Nachr.* No. 337) a memoir upon the astronomical uses of a lunar chart, in which he supplies formulæ for the calculation beforehand, in lunar eclipses, of the moment of ingress of the principal spots into the shadow's cone, as well as that of their egress, and has applied them to the total eclipse of October 13. 1837. In this memoir he also points out the spots which may be most advantageously examined in the determination of the precise position of the rotatory axis of the moon, and for ascertaining if this axis undergoes any real balancing as does that of the

9. *On the Effect of the Earth's Light upon the Moon.*—It is entirely to this terrestrial light reflected upon the moon that the authors attribute, according to the explanation of Leonardo da Vinci, which is generally admitted, the faint lustre which the obscure part of the moon presents at the commencement and termination of each of her periods, a lustre which renders the whole disk visible, and which has been named the *ash-coloured light.* The observations of Messrs Beer and Mädler completely confirm this explanation. The light, they say, diminishes so soon as the phase of the earth notably diminishes in regard to the moon, and disappears completely when the earth appears to the moon like a small crescent. It should appear brighter in proportion as the moon is near the sun, if the twilight of our atmosphere had not a qualifying effect. A *maximum* of visibility of the obscure portion of the moon results from a combination of the two causes just alluded to; and this occurs from two and a half to three days before and after the new moon. The visible spots exhibit precisely the same relative degree of clearness as when they are illuminated by the sun during the full moon. But, as our atmosphere is commonly not sufficiently clear for observations of this kind, as they must be made near the horizon, it follows, that the visibility and the glimmering light of these spots are but rarely witnessed; and this has led to the assertion, that volcanos have sometimes been seen in the moon, and that they are seen to burn in its dark portion. For ourselves, we have never been satisfied that we have witnessed any thing of this sort. It is common to speak of observations of this kind having been made by Herschel, an account of which may be found in the *Philosophical Transactions* for 1783. By

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earth, or if the whole effect of the libration is only apparent. He likewise demonstrates the utility of the knowledge of the moon's *relief* in the calculation by anticipation in solar eclipses and the oscillations of stars, of the moon's profile, and the effects of her inequalities upon the period of the commencement and the termination of these phenomena. In No. 338 of the same Journal, the author gives the result of the observations made during the eclipse of October 13. 1837, upon sixteen spots, which have given him an augmentation of the semi-diameter of the shadow equal to 1-54th part.

looking at a good chart, it may be seen that these remarks refer, without doubt, to three annular mountains, viz. Aristarchus, Kepler, and Copernicus, which this distinguished astronomer perceived, as others, and as we have often done, shining in the obscure part with a feeble ash-coloured glimmering light, and exhibiting in the telescope the lustre of a star of the fourth magnitude, as seen by the naked eye. Herschel, it is true, designated these under the appellation of *volcanos*, but simply, as he explicitly declares, because it was necessary to give them some designation, and not at all with the intention of explaining the appearances. Bode, Olbers, and Struve, all agree in thinking, that the variations which these luminous appearances present are owing to a difference in the circumstances of illumination and of libration; and that a burning volcano would not shew itself only in the obscure portion of the moon, when the terrestrial light is sensibly upon it, but in quite different circumstances when that light is in full activity. We may add, that it should shine brightly in proportion as the terrestrial light is less visible, when the surrounding region is more obscure, because real inherent light always appears most strikingly in the absence of all foreign light.

Schroeter imagined that he could perceive, that the ash-coloured light is more sensible before than after the full moon; and he conceived that this was owing to the difference of the reflection of the solar light from the earth, according as it was transmitted from the ocean or the continents. When (in Central Europe) the moon, a little before the change, appears in the morning in the east, the ash-coloured light proceeds principally from the great table-lands of Asia and Africa. When again, towards the evening, she is found in the west at the commencement of the first quarter, being confronted with the American continent, which is much narrower, and to the ocean, she must then receive much less light. Those observers who are situated within the tropics, and who can in all seasons easily distinguish the ash-coloured light as well before as after the new moon, and still more, those who can examine where the contrast of the oceanic and continental portions is greater than with us,—those, for example, situated at Canton and at Paramatta,—ought to direct particular attention to this point, seeing the great importance of such observations, among other reasons, for the knowledge they supply concerning the surfaces of different celestial bodies, and as exhibiting the comparative luminous effects of our oceans and continents observed at great distances.

10. *On the Meteorological Influence of the Moon.*—Here Dr Mädler successively reviews the previous researches of Messrs Schübler, Everets, Eisenlohr, Flaugergues, Boussingault, Alexis and Eugene Bouvard; to these he adds his own, the result of sixteen years' observations of the barometer, thermometer, and rain-gauge, made at Berlin six times a-day, and still continued by the author. The examination of these observations has demonstrated that, in this locality, the barometer has a mean elevation of $\frac{1}{3}$ of a line, and the thermometer of about $\frac{2}{3}$ of a degree of Reaumur, greater towards the apogee than towards the perigee of the moon, and that there is likewise a somewhat less quantity of rain or snow near the former epoch than near the latter; but the differences which the several years when grouped two and two together supply, are too great to allow an estimation of the numerical value of these results. As to the influence of the phases of the moon, the author finds that the greatest height of the barometer takes place at Berlin on the day of the new moon, the smallest two days after the full moon, and that the difference of height is 0^{l},928, with an uncertainty of 0^{l},297. It is sufficiently singular that these epochs should be different from those found in other places; the epoch which is generally indicated for the *maximum* of the height of the barometer being the last quarter, and for the *minimum* the second *octant*, or the eleventh day of the moon. As to the thermometer, which no one, we believe, has employed in these investigations previous to M. Mädler, he first finds that its *maximum* of mean elevation is 7°,73 R., and occurs two days before the first quarter, and that the *minimum*, which occurs three days after the fourth quarter, is 6°,72; supplying a difference of 1°,01, with an uncertainty of 0°,215.

The greatest rise and fall of the barometer most frequently take place towards the first quarter, and shortly after the full;

and the extremes of heat and cold occur less frequently between the new moon and the first quarter, than during the other parts of the period. But a much longer series of careful observations is required to verify phenomena of this kind, and to establish the law which they observe.

In investigating this subject, remarks M. Mädler, I was soon convinced that those great barometrical oscillations whose causes are still unknown, as well as the anomalies of the state of our atmosphere, must so inevitably mask the trifling influences of the moon, that we must needs for a long time renounce the hope of obtaining any permanent results from the observations made in our higher latitudes. The extent of the barometrical variations, according to the preceding observations, is 26 lines at Berlin (comprised between 321 and 347 lines, at an elevation of 130 French feet above the ocean); and those of the thermometer in the shade amount to 53° R. (viz. from — 23° to + 30°); there are some whole years in which the diurnal period of the barometrical height is nearly entirely disfigured: since a single considerable oscillation, and we have witnessed 14 lines in twelve hours, may notably change the form of the annual curve. These immense inequalities disappear near the tropics, and there the extreme oscillations are reduced to two or three lines, and we can every day recognise the solar period. I have endeavoured, therefore, to procure good observations made in the neighbourhood of the equator,—and I have obtained, by the kindness of States-Councillor Schumacher, a complete copy of those made five times a-day, from the 20th February 1829, to the 31st of January 1833, by Messrs Trentepohl and Chenon, at Christiansburg in Guinea, at the latitude of 5½° N. The barometrical range is so constant in this place, that each observation, after the usual reductions, and the necessary corrections from the known periodical variations have been made, scarcely departs, except in a very small number of instances, even a single line from the general annual mean.

M. Mädler, after reporting the monthly means of these observations for every hour when they were made, draws the conclusion, that in this locality two periods of barometrical variations exist, one dependent upon the sun's diurnal movement, and another upon his annual: the hottest period of the day and year corresponds to the *minimum* of the height of the barometer, and the extreme period during the diurnal period reaches 1,1 lines, from 9 h. to 4. He then examines the influence of the phases, and the variations from the declination and the distance of the moon. The smallest barometrical height corresponds to the second day after the full moon, and the greatest to the new moon, as at Berlin; the difference is only $0^l,194$, and the uncertainty only $0^l,06$. Although the effect of the change of the moon in declination should not be very sensible in latitude 5°, it nevertheless reaches $0^l,289$; and the regularity of the progress of these results appears to leave no doubt as to the existence of this variation. The *minimum* height corresponds to the greatest northern declination, and the *maximum* occurs two or three days after the descending node. Regarding the differences arising from those of the distance of the moon and earth, they are in the same directions as those found in higher latitudes, but they are too small to be susceptible of a precise valuation from so small a number of observations. M. Mädler concludes from these researches, that we cannot refuse to the moon some influence upon the earth as it regards its climate, although this influence is very small, and subordinate to that of the sun, and to other causes of variation which are still unknown. He admits, among others, that those years in which the moon attains its greatest northern and southern declinations, ought to be comparatively more favourable as to weather, and particularly for the cultivation of the vine, than those in which it is less removed from the equator. Finally, he thinks that the general laws of gravity are insufficient to explain these effects, as much in regard to quality as quantity, and that the properties of the moon's light, which we know theoretically, are still more so.

Remarks on some prevailing Misconceptions concerning the Actions of Machines. By EDWARD SANG, Esq. Communicated by the Society of Arts for Scotland.*

IN the last paper which I presented to the Society of Arts, I endeavoured to correct the common notion, that the performance of a model is representative of that of a machine. In this paper I proceed to expose the fallacy of some prevailing notions concerning the actions of machines in general.

The fancy that machines are capable of generating power, though fostered by a very absurd proclamation from our government, is now almost entirely abandoned; and, except from two or three individuals ignorant of the history and of the principles of mechanics, we hear of no attempts at obtaining the reward offered for the perpetual motion. But another fancy, differing less from this than it at first appears to do, is very generally entertained. We are perpetually told of the loss of force which arises from the obliquity of the actions of machines, and are called upon to examine cumbrous and expensive contrivances for rendering these actions direct, and for regaining, or even more than regaining, the force that has been wasted. If any arrangement of machinery were capable of destroying force, putting friction out of view, the inverse action of the same machinery would be capable of generating it.

The truth is, that every machine, however ill contrived, and however ill constructed, delivers over the whole, and exactly the whole, amount of force which put it in motion. Part of that force is expended in overcoming the friction of the rubbing surfaces, and in encountering the resistance of the air, while the rest goes to produce the particular effect which the contriver of the instrument may have had in view.

The geometric contrivance of machines, the arrangement of the parts so as to produce particular motions, has unlimited scope. But in a mechanical point of view, the inventor can, profitably, direct his attention only to two objects—economy in the material, and labour necessary to the first construction of the instrument, and the diminution, as far as is practicable, of the effects of friction. Were all friction avoided, it would be a matter of absolute indifference by what means the required changes of motion might be produced; it would then be of no moment whether we employed the reciprocating or the rotatory steam-engine,—whether we used the crank or the sun-and-planet wheel,—farther than the mere expense of workmanship is concerned. And we have no other criterion for estimating the superiority of one contrivance over another than the comparison of the amounts of friction in the two cases.

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* Read before the Society of Arts for Scotland 9th January 1833.

These statements will be startling enough to persons half acquainted with the nature of machinery. "What," they will ask, "does the obliquity of the crank cause no loss of force! Is there no force wasted in producing the reciprocating motion of the beam in the steam-engine! And were they mere dreams that we entertained of immense improvements in machinery?" And when I assert, what is well known to every one acquainted with the subject, that there is no loss of force from the oblique action of the crank, that there is no force wasted on the reciprocating motion of the beam, and that the removal of friction is the only source of improvement in machines for transferring power,—I oppose the prejudices of multitudes who ought to be better acquainted with the principles of mechanics.

The principles which regulate the balance of pressures, and the movements of bodies, though discovered by man, are not of human invention; they are laws impressed by the Omnipotent upon the material world—laws to which matter yields an implicit, a perfect obedience. These laws are few in number, and the simplest language in which they can be expressed involves the very statements that I have made. To exhibit, then, the truth of these statements would be to examine the reality of the fundamental laws of mechanics. To this examination I will not proceed, but will content myself with founding my investigations on the more common forms in which these laws are recognised. They naturally divide themselves into two classes: those which relate to the pressures of the acting parts during any momentary state of the machine, and those which relate to the properties of the machine considered as in motion. Those of the first or statical class, though at first sight very numerous, are summed up in one law, called the *principle of virtual velocities.* My first object will be to exhibit this principle in a clear light; not, indeed, as I would do in a scientific treatise on the subject, but in such a manner as appears to me to be best fitted for removing those prejudices, the existence of which has occasioned this paper.

When two weights balance each other by means of the wheel and axle, it is well known that they are to each other inversely as the diameters; but if the machine be turned a little round, the distances through which the weights move are directly as the same diameters,—so that, if each weight be combined with the distance through which it moves, the two results are exactly equal to each other. The descent of one pound through ten inches would, for example, be accompanied by an ascent of ten pounds through one inch, and thus whatever is gained or lost in intensity of pressure, as much is lost or gained in distance. It will be readily seen that the same thing is true of the straight lever, and of those combinations of pulleys which have their strings parallel: but in the case of the *inclined plane*, of the *bent-lever*, and, in general, of all machines in which the relations of the pressures are altered by a change in the position of the instrument, the application of the same rule is not so easy; and the slight difficulty that attends it has elicited the assertion, that the principle of virtual velocities is there at fault.

The relative motions of the different parts of a machine can easily be deduced from its geometric properties. The principle of virtual velocities enables us, from these motions, to compute the forces, and thus connects the geometric with the mechanical properties of machinery. This principle, one of the most beautiful and most pervading in nature, may be thus expressed:—

If the position of any machine be slightly disturbed, and if each pressure which has yielded be combined with the distance through which it has yielded, and each pressure which has advanced with the distance through which it has advanced, the sum of the one set of results will be exactly equal to the sum of the other set.

Now, in the case of the inclined plane, says the objector to the reality of this law, the weight raised and the weight which raises it move over equal distances. The motion of the weight is here mistaken for the distance through which its gravitation is overcome; its absolute motion, for its motion in altitude: now, it is well known that the force requisite to drag a body up an inclined plane, is to the weight of that body as the height of the plane is to its length; and therefore the principle of virtual velocity is here adhered to. This misconception is palpable and easily corrected; the motion of the machine does not alter the proportions of the forces. But, in the case of the bent lever, the error must be more involved, since it has crept from the workshop to the lecture-room, and has been promulgated where sound knowledge and accurate ideas ought to have rewarded the labours of the student. The arcs, we are told, which the ends of the arms describe, are proportional to the lengths of these arms, while the balancing forces are not proportional, inversely, to the same lengths. Here, again, the absolute motion of the point of attachment is mistaken for the distance through which the pressure is overcome. If, however, the objector's capacity be not entirely exhausted by the immense profundity of this remark, he will reply, and with justice too, that even estimating the motions in the directions in which the pressures act, the rule does not hold good.

For the removal of this mighty difficulty I must summon all my strength. Accustomed to handle tools (I speak not for myself only, but for every devotee of true philosophy), accustomed to handle tools admirably adapted for facilitating researches of this nature, and now called upon by the circumstances of the case to lay these tools aside and to venture on the question with unappareled hands, I cannot altogether divest myself of repugnance to the task. To render it, however, more inviting, I shall make the question as general as possible, and, without confining myself to this or to any other individual case, apply my remarks to those machines generally, in which the forces vary with the positions of the parts.

Conceive that for a given position of such a machine, the pressures are so arranged as to balance each other, and then let the machine suffer a displacement. If this displacement be considerable, the equilibrium will be materially disturbed, and the estimate of the motions, far from giving information concerning the original forces, would commingle the means for determining the forces in all positions between the first and last, and would rather be the basis for determining the conditions of the mean state, than of either of the extreme ones. However small may be the displacement, still will there exist an error in the estimate; yet the more minute the displacement, the more inconsiderable will be the error, because there will exist the less difference between the two extreme states of the machine. In order, then, to compute the pressures accurately, we must determine the proportions which subsist between the motions of the parts, supposing that these motions are infinitely small. I know of no other method for effecting these computations than that contained in the Differential Calculus, or in the more abstruse but more satisfactory theory of Lagrange; and, although the name of fluxions be a bug-bear to thousands, it is absolutely impossible, without its aid, to advance beyond the threshold of mechanical investigation. The advocates for mental indolence may urge that the statical properties of many machines may be examined without the aid of the fluxional calculus. Such is, indeed, the case, and these machines and such methods of calculating concerning them may afford lessons to the beginner; but from such processes few, if any, general conclusions can be drawn, while the instant that motion is contemplated, all these resources fail.

The principle of virtual velocities, such as I have defined it, holds true of all machines, even though subjected to the retarding influence of friction; and is applicable not merely during a momentary state, but also to the motions of machines. Denoting by the word *force*, the result of the combination of a pressure with the distance through which it acts; if the sum of the accelerating forces be just equal to that of the retarding ones, the quantity of motion in the machine will be unaltered, but if the two sums be unequal, then will the speed be changed. Now, in almost all machines the forces and velocities are subjected to periodic variations; the method then of computing the change in velocity consequent upon a change of force must be clearly understood, ere we can obtain any information as to the general properties of machines in motion.

The great proposition which connects the statical with the phoronomic properties of machines is this; that the change in the entire quantity of motion is proportional to the difference between the separate amounts of the accelerating and of the retarding forces; the quantity of motion being estimated by combining each moving mass with the second power of the number which represents its velocity. Now, I have already said, that, whatever be the nature of a machine for communicating force, the amount of force delivered during a minute instant of time at the one end, is exactly equal (throwing out of view the friction and the weights of its parts) to that communicated during the same instant to the other end; so that the same change is produced in the entire quantity of motion, whether the force be applied directly to the moving mass, or whether it act upon it through the intervention of machinery. This most important principle I shall endeavour to illustrate by example.

Borrowing my illustration from the steam-engine, I shall suppose one in which the piston acts directly upon the fly-wheel, by means, say, of a double rack working alternately on each side of a toothed wheel. Here it will not be denied that the accession to the quantity of motion in the machine during a half-stroke will be exactly what is due to the agency of the pressure of the steam upon the piston through the whole length of the cylinder, less, of course, by all the amount of all the retarding forces during the same period. Contrast this with what happens in the common steam-engine. Let the connecting rod be so nearly in a line with the crank, that a pressure of one hundred pounds on the piston exerts only a pressure of one pound in the direction of rotation; then does it follow, from the principle of virtual velocities, that if the piston advance minutely in the cylinder, the extremity of the crank will advance one hundred times as far along its path. The quantity of motion generated in the machine then will be what is due to a pressure of one pound acting through one hundred times the advance of the piston, or, what is the same thing, to the pressure of one hundred pounds acting through that advance itself. The same thing is true for every other minute motion of the piston, and, therefore, the whole amount of motion communicated to the machine through the crank is exactly equal to that communicated to it by means of the double rack and toothed wheel, or by means of any other contrivance whatever.

In the two arrangements, however, which I have contrasted, the motion will be divided among the parts in very different proportions. The manner of that distribution vitally affects the economy of the machine. The entire quantity of motion is not and cannot be concentrated in the fly-wheel; the piston and all the other moving parts must have their share of it. In the engine with the double rack, the piston and its appendages possess, from the beginning of a half stroke until the end of it, their full velocity. During the half-stroke, therefore, none of the force of the steam is expended in generating the motion of the piston; but just when the piston has reached its extreme position, its whole velocity must be extinguished and generated in the opposite direction, and the consequence is, that the extreme tooth of the rack will receive a blow as if from a hammer as heavy as the piston and all its appendages, and moving with twice its velocity; and although no loss of force would arise from this action, its continued repetition would tear the machine to pieces. In the case of the balanced crank-engine, on the other hand, during the first part of a half-stroke, the velocity of the piston and beam is gradually increasing; at the middle their velocity is the greatest; and during the last part it gradually decreases. In the first quarter revolution of the crank, the force of the steam is thus partly expended in producing the motions of the piston, beam, and connecting-rod; but in the second quadrant, when these motions are being retarded, the force necessary to accomplish this retardation communicates an equal accession of motion to the fly-wheel. Still, then, is the whole force of the steam expended in overcoming the friction, and in producing the particular effect which may be wanted; still is there no force lost on account of the obliquity of the actions, or of the reciprocation of the movements; and still is the diminution of the friction the only source whence increased effectiveness can be obtained.

I do not expect that what I have just said will be sufficient to eradicate misconceptions so prolific of crude and abortive schemes. The failure itself of the contrivance is often inadequate to convince its inventor of the fallacy of his ideas. Fortified in his ignorance by the fancy that theory is a different thing from practice, there is small chance of his yielding to the arguments of one whom he considers as a pure theorist. The fashion of the day, which puts diffuse and indistinct notions in the place of true learning, which cries for science levelled to the meanest capacity, or, as I would translate it, to the most confirmed indolence, fastens these prejudices more firmly on the minds of the half instructed. Were the evils of this fashion to rest with those who turn to philosophy for relief from the ennui of idleness, they might have been passed over in silence; but when they reach that class to whose usefulness extensive information is essential, their removal becomes an object of the highest importance. It would not be proper for me, in the present paper, to venture into the depths of this subject; but the connection between what is called *popular science* and many of those mistakes which are so current, is too immediate to permit of remarking on these without casting a glance, at least, to their fertile source.

Were the laws which regulate the phenomena of the universe, laws of human invention, and did they involve contradictions and absurdities, then, indeed, with some propriety might the cry be raised, "They are too abstruse, they are too difficult; let us have them simplified and levelled to the meanest capacity." Level these laws, and they are no longer the laws of nature; the true method of seizing them, is to nerve the mind with higher powers, to infuse into it an exalted ambition, and to come to the attempt prepared for long-continued and strenuous exertion. Would effeminacy pave the way to the white summit of the Jungfrau? or, had your parlour hearth-stones been brought from the summit of Ben-Lomond, think you that your delighted eyes could thence have wandered over lakes and mountains? No. He who would scan the wonders of Nature, who would contemplate the wisdom, the beneficence of her works, and would use his acquirements for the advantage of his race, must give himself enthusiastically to the pursuit, and must scorn to turn from difficulties in his path Perseverance will crown his exertions with success; and the elevation of his mind, the calm and ineffable delight which accompanies the acquisition of knowledge, will, a thousand times over, repay all his exertions. From the throne of science he will descry connections and arrangements and sympathies among the passing events, and turning, with his colleagues, to the yet unscaled heights, he will emulously pursue the career of discovery. The present is a spirit-stirring time; on all hands have discoverers been at work; from north and south has the hitherto unpassed barrier been assailed, and already have the signals of the workmen on either side been descried by their fellow-labourers. Elated by the prospect of speedy success, they now redouble their exertions, and expect ere long to reassemble on a higher platform. The science of mechanics has long reared its head proudly over its fellows. Under its efficient guidance, its indefatigable votaries have estimated the weights and motions of the heavenly bodies, and carried into the highest department of the science an exactness almost superhuman. Now, however, the sciences of chemistry, galvanism, and magnetism, advance rapidly to take their stations by its side, and promise to rescue from the charge of inconsistency the great laws of mechanics.

The precision which reason assigns to mechanical phenomena, the precision which these phenomena exhibit when the planets, launched in unfilled space, perform their mighty evolutions, fails us, when we lower our contemplations to terrestrial objects. Here the perfection of nature seems to be marred, the traces of absolute exactitude to be effaced, as if some evil genius had thwarted the Almighty in his design, and sowed confusion where order was intended. All motions on the surface of the earth are soon extinguished, and there was no wonder that men, in the infancy of science, drew the conclusion that matter possessed a reluctancy to move. It required a mind of no common energy to burst the shackles which education and early experience had combined to rivet, and to oppose its solitary strength to the bigotry of false religion and false philosophy. Unaided, Galileo long maintained the contest, and, although at last the man fell, his doctrine was victorious. Since that time friction has been recognised as the antagonist principle, which opposes, and invariably succeeds in extinguishing, motion, which creates errors in the results, and uncertainty in the practice of mechanical operations. Far, however, from being the cause of the imperfection of machines, friction is essential to their very existence. Not a fastening would be secure, not a screw would hold, were it not for its pervading agency. Without friction this world would have exhibited a scene of indescribable confusion. Conceive for a moment, its influence suspended, and where would be progressive motion? how would we climb the steep, how, even, would we walk along the plain? The mountain masses, rushing to the plains, and not arrested even there, as now, but hastening along with undiminished speed, would leave no spot for vegetable, no safety for animal life: though dashed to powder by repeated blows, each particle would yet move onward, and chaos would be realized. Far, then, from friction marring the general design, it itself is one of the most admirable and most beneficent provisions which nature's God has made for the felicity of his creatures.

When we glance over the vast fields of modern science, and contemplate the harmony that reigns among the known laws, when we consider the ease with which geometry is engrafted on arithmetic, the perfect acquaintance with geometric laws which is exhibited in the contrivance of the mechanical ones, we cannot imagine that the law of friction militates against or annuls one really existing law of nature. Chemistry has acquainted us with the permanency and indestructibility of matter; mechanics has taught us that the entire amount of momentum estimated in any given direction, is absolutely fixed, and has indicated that, except where friction and chemical changes interfere, the total amount of motion in the universe is unchangeable. The recent discoveries in galvanism and electricity shed a new light upon the subject.

The combustion of the coals, the chemical union of the carbon with the oxygen in the furnace of the steam-engine, generates motion; that motion is extinguished partly by friction, and partly in effecting the disintegration of bodies; and it now seems more than probable that this rubbing and this subdivision of matter induces a state, and communicates that state to surrounding objects, which afterwards goes, in some distant quarter perhaps, to reproduce chemical changes preparatory to a new evolution of the like forces. Small, indeed, as that change must be when absorbed into the general mass, it is not on that account the less real. The mass of stone that has been torn from the quarry, and fashioned into this splendid town, is minute indeed, yet, undoubtedly, that transference has retarded the rotation of the earth, although our senses, aided by every contrivance of science and of art, be utterly unable to discern a trace of the change.

While, then, we zealously strive to improve our machines, and to remove the friction from the inefficient to be concentrated on the working parts, let us not repine that, after all our exertions, we are still compelled to resign a tithe of our labour to that influence under which alone it is possible for us or our machines to exist; and let us console ourselves with the thought, that, though our exertions be lost to us, nature has taken care that they conduce to the maintenance and well-being of the general system.

Theory of Granite, and the other Massive Rocks, together with that of Crystalline Slate; proposed in Lectures on Geology, in the University of Christiania in Norway, in the year 1836. By B. M. Keilhau, Professor of Mineralogy. (Continued from Vol. XXIV. p. 403.)

At several points on the boundaries of the great granite and porphyry districts, it happens that granite and syenite on the one side, and porphyry on the other, pass completely into each other. Further, we find also in some places gradual changes from the granite to the clay-slate. Moreover, we see the same from granite to the primary gneiss, and also many other transitions. We shall here bring forward only some of the most important.

The boundaries between the porphyry and the granite districts exhibit sometimes a confused intermixture of the respective masses, and sometimes true transitions; the latter proceed quite step by step, so that we pass perhaps for a distance of two miles (more than one-fourth of a Norwegian mile) over a rock intermediate between granite and porphyry, before we reach the clearly exhibited type of the one from the distinct type of the other. If the opinion be adopted, that the massive rocks in such districts are the products of eruptions, still it cannot easily be admitted that such enormous outpourings of two such entirely different materials should have taken place at one and the same time at points lying so near to each other. A priority must undoubtedly be assigned to the appearance at the surface of the one or the other. But in doing so, so far as I can see, we cut off the only possible approach to assigning, according to the eruption theory, a kind of cause for the transitions, viz. that the porphyry and granite on meeting at one time in a fluid state, became blended together at the points where they encountered each other.

But what may be still more unwelcome than the phenomena of the boundaries of granite and porphyry, to certain theorists, is, that we observe transitions from the *massive* to the *stratified* rocks. One of the greatest errors in modern geology, is the distinct separation, *proceeding from the genetic mode of viewing the subject*, between stratified, or what are termed normal formations, and abnormal formations. In the manner in which it is wished that this separation should be understood, it is in fact altogether contrary to nature; for, according to this system, there cannot and must not be any real transition thought of from the type of one of the two great classes to that of the other; and yet such a transition exists in thousands and thousands of places. This deviation from truth is its own punishment. Thus, we see some geologists who are under the necessity of denying a fact so clear as the transition of granite into gneiss; while we find others who rank gneiss, mica-slate, and I know not how many other stratified rocks, in the class of massive formations.

Let us examine the suite of specimens of mountain-rocks from one of the most instructive localities in our territory, that of Sölvsberg* in Hadeland. In the first piece, we recognise the usual siliceous slaty rock into which clay-slate and calcareous clay-slate pass when near the granite boundary, a rock which exhibits distinct petrifactions, and in which, likewise, stratification is particularly well marked. In another portion we still detect the parallel structure, but in the mass which in the preceding specimen seemed homogeneous, there now glimmer fine particles that in the following fragment are to be recognised as plates of mica, and in the succeeding one there also occur portions of felspar, and so forth ;—in short, there is to be observed a gradually advancing development from compact slate to a large-grained granular rock, which Von Buch has expressly termed granite. But, however convincing our suite of specimens may be, it is so but in a small degree, compared with the sight of these phenomena in nature itself; for on the spot we can distinctly remark this circumstance, that the transition is by no means sudden, but extends over a long distance. We can plainly see how gradually it takes place, and that thus the stratification and fossils disappear only by little and little, and all in proportion as the compact structure is converted into one which is more crystalline-granular; and I have no doubt that every one who will bestow sufficient time and attention on the investigation, will not only be able to follow the parallel lines of the undisturbed direction of the slate beds into that region where *granitification* has already distinctly enough made its appearance, but will detect traces of petrifactions where the siliceous masses present shining microscopic crystalline grains ; and the same is the case in specimens from another analogous locality.

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* The remarkable locality of Sölvsberg, mentioned in the text, is situated near the Rands-Fiord, and but a little to the west of the great road from Christiania to Bergen and Trondhjem.—Edit.

The boundaries of the granite and slate are not everywhere such as we have now described them. The occurrence of ramifications at the boundary is certainly the most usual ; but still it is not exclusively the appearance presented ; for at some points at least, instead of such a phenomenon, we find perfect transitions between the granitic formations on the one side, and the siliceous slates on the other.

Similar transitions can also be pointed out under entirely different circumstances. These are of importance with respect to an objection that might be made regarding the phenomena just described. The opinion has been started, as we shall see better afterwards, that felspar and the other component ingredients of crystalline slates have been developed by the operation of the melted masses occurring in their vicinity, *i. e.* by means of the same causes as have been supposed to have effected the conversion of clay-slate into siliceous slate. There are also to be found in our territory places where, in fact, the stratified rocks in contact with granite do not merely exhibit the usual degrees of alteration as flinty slate, striped jasper, &c., but at length at the very boundaries present imperfect mica and hornblende-slates, or even a sort of gneiss. But in these cases there is always present a marked line of demarcation between such slates and the massive mountain-rock. It must thus be admitted, that if the crystalline slate, which is in contact with granite, and presents a sharply defined boundary, is to be regarded as formed in the manner described, such an explanation can by no means apply to the case where a complete transition takes place; thus, for example, at Sölvsberg, we could not say that a part of the granite which there presents itself is directly pyrogenetic, and that the remainder, which affords transitions to the slate, is indirectly pyrogenetic, being produced by the action of the other on the slate; and we have here the peculiar circumstance, that where both crystalline mountain-rocks border on one another, they have so great a resemblance that the difference cannot be observed, and thus the boundary might easily be overlooked, should it not have been really obliterated by these having been melted together. Should any one now, further to support a preconceived opinion, come forward with such an interpretation of the transitions we have been considering, we have still another fact of an analogous kind to bring forward, which scarcely leaves the supporters of the volcanic origin of the rocks in question any other mode of escape than that of which we had formerly an example, namely, to deny the accuracy of the observations. The phenomenon of which I speak is the following: there are places (for example on the Langesund-fiord*), where the slate shining with microscopic crystalline particles, lies as a bed of only one inch in thickness, between other siliceous beds in which no crystalline structure has been developed, or between strata of limestone; in other beds of the same range of strata, and as little as the previous one in contact with abnormal rocks, but somewhat thicker than it, we see transitions from compactness to crystalline structure which have proceeded farther; and finally, again in others occurring under the same relations, but which are a couple of feet in thickness, we find masses that present a perfect crystalline granular trap, which, in these places, represents the common greenstone. From the trap-rocks there are transitions through petrographical intermediate links in the less and less considerable beds to the finest granular masses in the thinnest bed, all of which are distinct; and between what has been last mentioned and the common hard slates, there are found intermediate rocks sufficient to complete the suite. In other places, where bed-like masses of the usual greenstone occur between strata of clay-slate and calcareous clay-slate, there are found small thick beds of a rock having an intermediate character between clay-slate and greenstone; but so far as our observations have hitherto gone, the range of beds which exhibits the different shades of transition, is not so complete here as in the other peculiar example of trap, nor are these beds so thin as to preclude to the same degree the admission of the hypothesis of the lateral injections.

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* The Langesund-fiord is the next arm of the sea to the west from Frederiksvaern, and has on its shores the towns of Langesund, Brevig, and Porsgrund.—Edit.

The transitions from the granite of our territory to certain primary strata must likewise be briefly noticed. I confess that no geological phenomenon has struck me so much as this, that I found places where the boundary between the granite and the primary gneiss had entirely disappeared, where the nature of the rock changed completely from the characteristic type of the one rock to that of the other; so that at first I despaired of the possibility of finding any explanation. As to what regards the facts in this case, the relations are precisely the same as in the transitions from the same granite to the fossiliferous state. For the most part we find at the boundary ramifications of the massive rock branching into the primary strata; but sometimes, though rarely, transitions occur. That these transitions bear testimony against the eruption theory, in the same manner as the transitions between the granite and the newer strata, requires no further explanation; but, in passing, we may remark, how extremely important this same fact is as an argument against Neptunism. It appears that some of the supporters of this doctrine have endeavoured to explain the ramifications of the massive rock in the bounding strata, and generally the relations of the boundaries and positions of these rocks—relations which have of late been much more carefully examined—by assuming a simultaneous formation of these rocks which often occur together in such a peculiar manner. But after what has been said this mode of escape will not suffice; for should it be maintained that the granite of the transition territory had intruded itself at the same time that the stratified rocks were deposited, it might perhaps be said, with respect to the ramifications in the primary rock, that these were upfillings of the vein-fissures then open in the basis of the new formation. But ramifications in the primary rock, together with transitions into the same, might just as well imply a simultaneous origin with the last as with the new formation. But it is not our object to combat opinions which have been excluded from the science, and which can only preserve their name in its history. It is, on the contrary, those views which have stood their ground against all the hypotheses hitherto proposed that we have now more strictly to examine.

For this purpose it will be enough to present a group of facts. Every one who has, even with the smallest degree of attention, examined our territory, must have made the remark that the great porphyry districts are regularly to be found where sandstone occurs; and that eurite-porphyry is associated more particularly with alum-slate. Meanwhile, the supporters of the eruption theory may, perhaps, lay no great weight on these circumstances, which may appear to them as altogether accidental, and they will not feel themselves incited to investigate how far there exists a fixed place in certain stratified rocks of a particular kind for massive formations; for if we consider the latter as having burst forth from the interior, they might of course have made their appearance at any place whatever, without reference to the mineralogical constitution of the mountain-rock broken through by the eruption. That such an assumption, at least with respect to our massive rocks, is incorrect, and that it would prevent us from observing some of the most beautiful and important phenomena of our territory, we shall now have an opportunity of shewing.

I must first of all remark in general, that all those porphyry and granite formations which have their petrographical characters so distinctly marked, and which, therefore, are so easily recognised, are not at all to be found in any place in the whole country, except within the boundaries of the two transition territories; veins of greenstone only, more or less similar in composition to the greenstones of the transition series, are to be met with in other localities. It is thus perfectly clear that at least all the other numerous massive formations, which occur more especially in the Christiania territory, are most intimately connected with the group of transition-strata of which we are speaking. In Sweden, as we shall afterwards see, massive formations occur of the same kind as ours, and there they are associated with a sandstone like our own, with orthoceratite limestone, with clay-slate, &c. The *genetic* connection—for less it cannot be—between these two kinds of rock, the above-mentioned stratified and unstratified, is particularly apparent in looking over the map of the Christiania territory; for it is impossible to ascribe this connection to accident, and it would be a poor shift for any one to say that the places where the massive rocks in question occur, offered less resistance to the eruptions than any others.

Having premised these preliminary observations, we will now consider the subject in a more special point of view, by examining the distribution of the massive rocks within the limits of our territory. First of all, I should be inclined to lay particular weight on the constant association of the great porphyry masses, or rocks belonging to the great porphyry districts with members of the sandstone-formation, viz. those slates, quartzose sandstones and conglomerates, which occur in the upper portion of the stratified rocks of the group. It is not a mere conjecture, but a fact, that the presence of this sandstone-formation affords the condition required for the occurrence of the great porphyry masses. However poor the Upland territory may be in unstratified rocks, yet we soon perceive that this massive formation presents itself wherever these sandstones are visible; and, in the territory of Christiania, I know only one locality where this same porphyry is not associated with the sandstone. The stratified rocks are for the most part bounded by the granite districts; but wherever sandstones occur in the series of strata, there the granite and sienite immediately disappear, and porphyry now becomes the bounding rock ; a rule from which I know only one exception, and that is at the bottom of Sande-fiord, where granite comes in contact with quartzose sandstone ; but this case is accompanied by particular circumstances, into the nature of which I cannot at present more particularly enter. A similar association of transition-sandstone and porphyry has been observed out of Scandinavia ; and on this point, I need only refer to the porphyries which occur in such abundance in the great coal-formation. When we treated of the coal-formation, we did not venture to deduce, from the constant association of the massive rocks with certain members of the coal group, a connection by formation between the unstratified and stratified masses ; we merely remarked that such an opinion had previously existed. On that opinion we are now perhaps inclined to lay more weight.

If, in our transition group, the dependence of the porphyry on the sandstones has once been admitted, from analogy it cannot be denied that a similar dependence exists between the granitic rocks and the clay-slate. But the most striking of all is the connection between eurite-porphyry when it occurs in beds, and alum-slate. It is possible that this last union has an entirely different cause from that of the combination of the great porphyry and the sandstones. Alum-slate, for reasons which shall afterwards be explained, occurs especially near the primary rocks ; hence, if the bed-like masses of eurite-porphyry, whether from a general or special cause, belong particularly to the lower portion of the series of transition-strata, so will they, as is actually the case, be often found along with the alum-slate, without implying the dependence of the one formation or the other. But for the moment, let that be as it may, certain it is that eurite-porphyry or a similar rock as its representative, is *perfectly regularly* found in all the places where we see alum-slate in the neighbourhood of the primary basis ; there we find it more or less perfectly in the form of beds, quite as a determinate member of the formation, and alternating with the other transition strata. As already stated, it always occurs in the oldest portion of the transition-rocks, sometimes even directly on the outgoings of the primary strata ; so that (as is shewn by later denudations) it is only on the hanging side that it is connected with the Neptunian transition-strata. In regard to this subject, we have to mention a very remarkable circumstance,—that the petrographical characters of the rocks are modified in a determinate manner, according as the masses lie quite near the basis of the transition formation, or are found somewhat higher up in the series of beds. In the first case, it presents itself as almost a pure quartz, as an impure ochre-coloured rock, in which felspathic substances are very rarely present, and in which felspar crystals are often entirely awanting: but higher up in the range of beds, the basis or ground of the rock acquires more and more felspathic matter, and first of all becomes euritic, and afterwards quite crystalline. At a still greater distance from the primary series, the same rock becomes of a sienitic character, but then it can no longer be said to have a fixed place in the succession of strata. The regularity of these progressive changes is limited only by another likewise very remarkable rule, that the larger masses are more crystalline, and richer in felspar, as well in homogeneous distribution in the basis or ground, as in the crystals, than the smaller ones; so that a perfect porphyry may present itself very near the primary rock, provided the mass be large enough, while a compact and rather quartzose than euritic rock may occur further on in the range of beds, because it exists in a comparatively thin bed. That the larger masses are more crystalline than the smaller, accords perfectly well with Vulcanism; but certainly the other relations we have mentioned, cannot thus be made to correspond, for it seems incontrovertible that this remarkable family of the massive rocks,—whether in the form of quartz, or of sienite, or of true eurite porphyry, cannot have been intruded from without into our formation, since they could as well have appeared elsewhere, even beyond the limits of the formation; and it is also clear that they are associated in a peculiar manner, which banishes the idea that the masses belonging to them have taken the places where they are found accidentally.

These, now, are facts of a new kind, and it is a fortunate circumstance that they can be observed in our own immediate neighbourhood, although the full conviction of their accuracy can only be derived from the examination of a great number of places. I cannot sufficiently recommend the study of them in nature herself; for I am convinced that every person who finds the present account of them correct, will escape what, in my opinion, is the great error, of considering all mountain-rocks termed abnormal formations without distinction, as volcanic (Plutonian), *i. e.* masses, which were once erupted from the interior in a fluid state to the places in which they are now found.

Thus far advanced in our investigations, we must now prepare ourselves to answer the question, What is the origin of these massive rocks of which we have been treating? since we are not able, at least generally, to adopt even that one of the two prevailing theories, which seemed to be capable of explaining the greatest number of our facts. Are there any less commonly acknowledged views on the subject of which we might avail ourselves, or are we perhaps under the necessity of striking out for ourselves an entirely new path? It really appears to me that we are in the last of these situations. Of all the hypotheses hitherto proposed with any degree of distinctness regarding the origin of the unstratified rocks, I know only one which claims our attention in seeking for a theory of the granites, porphyries, &c. of the Christiania territory—it is that of Keferstein.* We shall afterwards have occasion to give a full account of this, but at present it is sufficient for us to know that Keferstein considers the massive rocks as the result of a spontaneous *vulcanisation* of certain portions of the Neptunian rocks, which being in a state of fermentation at particular points, were heated to fusion, or in general were brought to a condition for crystallizing, in consequence of which they occupied a larger space, and were upraised, &c. I treat the whole conception with respect; but Keferstein has done homage to the spirit of the times, and in spite of his heresy, he seems to wish to be classed among the vulcanists. These last, however, stoutly disavow him, at least they will never forgive his unlucky observations on fermentation. For our own part, we really lament that we cannot form an alliance with this geologist. As our reason for this, it will be enough here to state, that with us he must stand on the same ground as the vulcanists, inasmuch as he takes for granted the strata-disturbing influence of the granite. We must, therefore, endeavour to find for ourselves a position from which we may discover the causal connection, if not of all, at least of a greater number of the facts we have stated, than it is possible to do by the hitherto existing theories,—I allude chiefly to Neptunism and Vulcanism as the only ones which have as yet received any full exposition, and whose principles have been clearly set down.

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* A full exposition of M. Keferstein's curious views will be found in his work entitled "*Die Naturgeschichte des Erdkörpers nach ihren ersten Grundzugen.*" 2 vols. 1834.—Edit.

Before we go into the subject itself, it becomes necessary to obtain a clear view of our position, and to consider especially our relation to chemistry. With this science geologists are most intimately connected; and in a particular manner, the theory of granite and the other crystalline mountain-rocks, stands very closely allied to it. That the truths of geology and chemistry can never be found in contradiction to each other, lies in the nature of things. But both sciences have the same rank: we must not, as would almost seem to be required, ascribe such a supremacy to chemistry, that the geologist should not be able to step beyond the point of development corresponding to the stage in its progress at which chemistry may have arrived. The contingency may readily be imagined, of a subject being extremely problematical in chemistry, with regard to which we may be able to form a decided opinion by the aid of geognostical facts; and that, while a geologist directs his attention to what has taken place, or is now going on, in the great laboratory of nature, he may thus discover phenomena, and unfold ideas connected with the province of chemistry, which he could not reach in the workshops of art. It would, therefore, be quite in the order of things, that the geologist, by his theories with respect to granite and the other massive rocks, should advance before the experimental chemist; and even if it should chance to the latter to produce a granite perfectly similar to that which is formed by nature, the geologist is not absolutely bound to admit as the process of formation of the natural rock, that which took place with regard to the artificial product, when he finds that mode of formation at variance with geognostical phenomena. The mode of theorizing with respect to granite, &c. has been this,—that an hypothesis founded on chemical principles was carried as far as possible, but for the rest, geological necessity was allowed to reflect back arguments for the justice of the assumption on what remained as a chemical postulate. So long as the opinion was entertained that granite, in its geognostical relations, coincided with the rocks deposited from water, it was natural to suppose that it also was derived from a Neptunian fluid. Granite is an aggregate of crystalline minerals. The formation of crystals, on the expulsion of fluid matter existing in the form of water which held them in solution, is the most common way in which we see these derived, not only morphologically, but often also substantially as newly formed bodies which we call crystals. The circumstance in which we here went beyond chemistry, was the assumption, on these grounds, of the formation of felspar, quartz, and mica, in the moist way.

In the same way has it fared with the now prevailing theory. As soon as it was discovered, in consequence of our being better informed with regard to the geognostical relations of unstratified rocks, that these relations could not be brought to harmonize with their Neptunian origin, the Neptunian hypothesis was cast aside, and replaced by a new one,—one known for a long time previously, but that is of no consequence for our argument: crystals are also produced from melted masses; and although crystallization is much more frequently observed to take place in the humid way, yet, in that group of minerals which compose the massive siliceous rocks, according to certain facts regarding many of them, it could more especially take place when their materials existed in a melted condition, and from that passed to a compact state. With regard to granite, chemistry has actually advanced so far as to be able to produce, in the dry way, felspar and mica; and, so far as one may wish that the chemical postulate of the pyrogenetic theory may pass over entirely to chemistry, it is only wanting that we should be able by fusion to procure quartz, and to produce all the three minerals in combination. But to us it would be a matter of little consequence the success which might attend such attempts.

The pyrogenetic hypothesis does not satisfy the demands which a mass of geognostical facts have on a theory of the rocks in question, and the inquirer must look about him for a new hypothesis. So far as is practicable, chemical experiments must be attended to; for, as we have recognised as a rule never to be deviated from, the geologist in his postulates must always have it in his eye, that, sooner or later, these must be brought to accord with chemical principles; so that it is not till, in his search after a theory, he has failed in obtaining assistance from his more or less perfect acquaintance with the formation of crystals or analogous bodies, that he should consider himself at liberty to go beyond all direct experiment.

After these general remarks, we must proceed to inquire in what other way than that proposed by the Neptunists and Vulcanists, such formations could take place as may afford analogies, or at least hints for the solution of the problem as to the origin of granite, syenite, &c.

In following up this subject, the first thing that presents itself to us is the well-known phenomena of the crystallization of substances which occur in the form of vapour or gas; but beyond this a dark region lies before us, into which, however, we must penetrate. It appears that mineral bodies, or to speak generally, solid unorganized bodies, are capable of changing their morphological and chemical characters; that is to say, they present themselves in forms which did not exist before in their place, and exhibit a combination of chemical constituents different from their former one, or in short appear as new bodies in both respects, without necessarily requiring that this change should be preceded by their having been in a fluid state. I say it *appears* that this is the case, for that a movement of the whole, or at any rate of part of the substance of which the bodies produced in this manner consist, and in the case of newly formed crystals at least, a very free movement must be supposed to have previously taken place; and the possibility of this we can only conceive in a state of fluidity. It is nevertheless a fact that formations take place *partly with a material*, which, to our observation, was, and continues to be, what we call firm and rigid, that is, neither in a liquid nor in an elastic state of fluidity; and *partly in a solid medium*, which, according to our common ideas, seems not capable of allowing any movement of the substances in it. It is extremely unfortunate for geology that the whole of this field for important investigation has been left almost quite uncultivated. Thus there exists a metallurgical process which exhibits in a striking manner the fact we have mentioned, but which, though it has been long employed, and therefore must have been for a considerable period known to chemists, has yet never been accurately examined. I allude to a certain mode of treating poor copper-ores, described many years ago by the Italian geologist Breislak, and employed in Scandinavia as well as at Agordo in Italy. It consists in bringing, by a proper degree of heat, but which, in order that the process may succeed, *must not go the length of causing smelting*, the greater part of the pure copper into the middle of a mass of ore, while portions of the iron and sulphur contained in the crude ore seem to be driven outwards. The copper, previously only in the state of copper-pyrites, which was more or less uniformly disseminated through the whole mass, is now found in the interior as a kernel, and exhibits a compound resembling variegated copper-ore, or a mass even richer in copper. Give this phenomenon what explanation you please, still it unquestionably belongs to that group of facts which teach us the possibility of such considerable chemical and morphological changes in what are called rigid bodies, that, in the hypothesis we seek for, we are not under the necessity of taking for granted, that, for example, the substance of granite, immediately before the formation of that species of rock, must have existed in a fluid state, whether in a Neptunian liquid condition as a melted mass, or in general in any of the modes of fluidity hitherto commonly known; for if it should be said that the case we have quoted does not exclude the possibility that the transposed substances might have been in a state of elastic fluidity, yet, even if this could be shewn, the conceptions we have hitherto formed with regard to this state must be not a little modified, namely in this, that we must consider substances in this form as much more adapted to penetrate and to move in solid masses than we have hitherto believed them capable of doing. The following are also examples of the processes of which we speak, viz.—of the crystallization of and in solid masses: the transition of compact substances, such as glass, amorphous limestone, &c. into a crystalline state by the action of a moderate heat; and the conversion of barley-sugar from an uncrystalline to a crystalline condition; of chemical changes without crystallization: the conversion of various minerals into steatite (*Speksteen.*)

When we examine more closely the phenomena of these chemical and morphological formations, we are led to the conclusion that the heat which is in operation in these processes, is of use chiefly in hastening them, inasmuch as it imparts to them a greater degree of intensity, and thus appears as an indispensable cause. Where, therefore, in the example borrowed from a metallurgical process, new combinations of copper and sulphur were, by the assistance of heat, induced in a space of a few weeks; there, according to the data afforded by various observations on Roman antiquities, which have undergone analogous and still greater changes of composition, some thousand years would have been requisite in order to produce such alterations at the ordinary temperature.

It is quite evident that we must anticipate chemistry when we are forced to rest on facts like these quoted, because chemists have hitherto bestowed too little attention on such phenomena, inasmuch as all the materials furnished by that science for our discussions consist of a knowledge of the composition of crystals and mineral species. It would, first of all, be requisite to prove if the well known fact of the formation of crystals from expansible fluid bodies, could here be brought to bear on the subject. The first glance even shews us that the consideration of this phenomenon would only lead us into an entirely unproductive field. We may take this opportunity of mentioning an hypothesis, proposed at an earlier period, and which may be quoted as an example of, so to speak, the despair of clearing up the difficulties as to the unstratified mountain-rocks. Among the efforts to unravel this great geological riddle, the attempt was made to consider some at least, of these formations, as the result of the condensation of aëriform matter which was dispersed through the atmosphere, and which was afterwards precipitated and assumed a solid form. To follow up this idea any further would be but a waste of time.

There is another view which is of greater importance. The opinion has been proposed, that emanations of substances which are in a condition to crystallize, can, by penetrating solid bodies, deposite crystals in them. As in discussing this subject authors—to the injury of science—have been inclined to express themselves in an undecided and obscure manner, I really do not know with certainty if any one has imagined that, for example, a compact slate *alone* can in this manner be converted into gneiss, and if a sort of smelting at the same time is assumed to be also necessary. From what we have said of the theories of the origin of dolomite and gypsum, it is evident that the idea is current, that at least some of the component parts of mineral bodies can pass through solid masses, and that they can even unite with other masses in the solid form, in order, in this manner, to cause the formation of new chemical combinations, and of crystals. Although no one has thought of any other mode of existence of the matter supposed to be moveable, than that in the form of a gas or vapour, yet still we perceive that we here find ourselves entirely on the ground we last entered on, when we inquired after the imperfectly known modes of formation of mineral species. But be the fact what it may, it comes to the same thing whether we assume that these moveable matters, as they are considered, are in the state of known expansible fluids, or are supposed to exist in a still more subtile form; we can scarcely hope to penetrate into these mysteries, and in the mean time these ideas remain alike available or alike unavailable as to our problem.

This subject ought to attract our attention in a very high degree, because the outlines of the granite masses, the passage of the granite into the stratified rocks at the boundary, the ramifications we observe at other points, &c. remind us very distinctly that all these phenomena, even to the minutest detail, are only a repetition of the appearances presented by dolomite in its relations to surrounding stratified rocks. An esteemed geologist, Mr De la Beche, remarks that so soon as we have an accurate knowledge of the relations of position of a mountain-rock, we cannot be much in doubt regarding its mode of origin. I have always regarded this as an important truth, and it follows from it as a consequence, that two mountain-rocks which agree in this respect, must also have had the same mode of formation. I think, therefore, that the same theory that is applied to the origin of dolomite, ought also to be employed in regard to granite and other analogous formations.

The dolomite theory supposes that the dolomite has been produced by the penetration of magnesia exhalations into masses of carbonate of lime. There is nothing in our previous remarks which can be directed against the admissibility of this theory; but it certainly anticipates the data afforded us by chemistry. It is not satisfactory to me, because it involves other extremely arbitrary assumptions. It cannot be denied that in some places dolomite occurs in very thin but extensive strata, and quite in a regular manner between other mountain-rocks; now how is it to be explained why the sublimed matter left all the other strata untouched, and only exerted its influence on the carbonate of lime in thus converting it into dolomite? In the *irregular* masses of dolomite, which are completely surrounded by Neptunian strata, there is a similar but less considerable difficulty to be solved. But, not to state other objections, I shall merely mention what seems to me the greatest, that the whole emanation-hypothesis reposes on Vulcanism; for if the black porphyry (melaphyre) or other massive mountain-rock, with whose intrusion dolomization has been put in connection, be not pyro-genetic, then the occasion of such an extraordinary mode of formation gives way, and we are just as little able to assign another cause in its stead; and indeed it ought only to be in the case of extreme necessity that we should have recourse to the theory of emanations.

In the mean time it may be remarked, that whoever, by adopting the dolomite theory, admits all these suppositions, must necessarily find no insuperable obstacle to admitting that the materials for the formation of felspar, quartz, mica, hornblende, &c. may, in like manner, be conveyed by exhalations, in so far as was required at the places in the already existing masses where the development of these minerals should be assumed to have gone on. If we assumed that these conveyers of matter rose up from beneath, then we could say of the great porphyry masses in our transition district that they were their result, that the emanations had established themselves in the layers of the sandstone, that, on the contrary, the substances necessary for the formation of eurite-porphyry had not been elevated to that height, but had united themselves to certain strata in the vicinity of the primary rocks, and so forth.

But, in considering the subject more closely, we may leave out of consideration all these artificial speculations, and do not require to think of emanations, whether with reference to dolomite or to crystalline siliceous rocks. We should otherwise be led into a dark region, which will not for a long time be illuminated by the light of chemistry. It can be of very little service to trouble ourselves with hypotheses wherein chemistry teaches us to respect its operations, but of which it shews us that not one half can be made available. It is, therefore, a much more natural proceeding for us to confess candidly, that the moment is not yet come when we can give a full and satisfactory account of the phenomena observed, and that we should, therefore, as yet limit ourselves to the examination of the facts, such as they are, which are the first steps to an explanation, without for the present entering on the more remote causes.

We are undoubtedly in possession of the following two most important facts :—First, that masses in a state of compact aggregation can crystallize or undergo morphological changes, without, so far as we see, previously acquiring, in any degree, a fluid condition ;* and next, that substantially newly formed bodies, crystallized or uncrystallized, can be developed in compact masses likewise, without our remarking that the matter, immediately before the formation of the new body, was melted in any of the known methods. These two facts are all that we require, but they are absolutely necessary to enable us to explain a very large number of geological phenomena, both small and great,—so much so, that it really appears quite extraordinary that proper attention has not been bestowed upon them. For my own part, ever since I directed my attention to these, though obscure, yet very important facts, I have constantly endeavoured more and more to shew their value in the science of geology. Even in 1828,† I endeavoured to advance this position,—"THAT ONE OF THE HITHERTO KNOWN FORMS OF AGGREGATION IN WHICH FLUID BODIES EXIST, DID NOT NECESSARILY PRECEDE THE FORMATION OR TRANSFORMATION OF BODIES; BUT THAT, ON THE CONTRARY, THE COMPACT FORM DID NOT PREVENT MOVEMENTS IN THE SUBSTANCE." I shewed "the possibility that in the solid portions of the globe very essential changes, in consequence of the moveableness of the particles of solid bodies, may have taken place, and still continue to take place." "And," I added, "can any one be afraid in this case, to form the conclusion that such an action is possible?" There is much in modern geology which shews us that we must abandon the unauthorized supposition "*that the parts of mountain-masses, with respect to their specific constitution, and the position they now occupy, are every where the same as at the moment, when, at a former time, they or their materials were produced from a gaseous or melted condition, or from solution in a Neptunian menstruum.*" But we still find that this belief is adhered to; and it is only by casting off such ideas that we may hope for "some light being perhaps thrown on the origin of so many singular arrangements of rock-formations, which, by the assumption of an absolutely inactive state of the solid portions of the globe, cannot be at all understood." I also stated it as my opinion, that "a multitude of mineral products whose formation has in a very forced manner been attributed to infiltrations or sublimations, will prove to be the result of long continued processes in the compact stony mass." A continued study has more and more strengthened these earlier views: I regard them as the foundations of the whole of geology; and they are strongly confirmed not only by the consideration of the mineral masses existing on the great scale as the component parts of mountains, but also by the investigation of the varied modes of their occurrence as immense veins, beds, imbedded masses, &c. together with the examination even of imbedded and disseminated minerals.

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* Gay Lussac, after narrating the well known fact regarding barley-sugar, expresses himself in the following manner : " Ceci prouve que, dans un corps solide, les molécules peuvent changer de position et prendre la forme crystalline. Cet exemple a fait dire que les molécules des corps solides peuvent, dans certaines circonstances, prendre de nouveaux arrangemens. C'est qui a lieu pour un grand nombre de sels. . . . . . Ainsi les molécules des corps solides ne sont pas tellement liées entre elles, qui'elles ne puissent changer de place et former d'autres groupes."—(Cours de Chimie, II. 26, Leç. 24.)

† Poggendorff's Annalen, xiv. 134.

I must again repeat, that we are not to be prevented from adopting a theory because the facts on which it rests cannot be further explained, for we are not inclined to discontinue all the geological investigations belonging to it, until we approach more nearly the desired solutions of our difficulties, nor can we longer occupy ourselves with the old, hitherto exclusively cultivated ground, where new resources no longer present themselves for new necessities.

We shall therefore endeavour to avail ourselves as much as possible of the facts we have announced. When we consider directly the grounds on which we repose,—in such a manner that we avoid all idea of penetrating the subject to the root or foundation, but, separating every reflection tending to obscurity, regard them only as mere phenomena,—we may be led to some general conceptions, in which errors possibly exist, but which are, nevertheless, to be received, at least provisionally, as correct. Thus in certain cases we may imagine, that the materials which are necessary for the formation of the new bodies were not derived from any points *without* the parts of the solid masses where formations take place. Keferstein, when speaking of steatites, (spekstenen) and other similar metamorphoses (properly speaking *metastomatoses*), remarks quite correctly, that the chemical constitution of these minerals has not the slightest connection with the chemical composition of those species by whose transformation they are produced, and shews that we can only assume *that the chemical substances have been completely converted into one another*. Until chemistry can enable us to trace up more successfully the nature of the process which goes on in the instances mentioned, we may adopt the same idea and retain the same expression. The first of the general conclusions referred to is therefore the assumption of the possibility of perfect *substantial transformation*, by which it is supposed that the conveyance of matter from without is not necessary for the formation of new bodies. It would therefore be regarded as not at all impossible that a mass of pure carbonate of lime, without the addition of magnesia, and without the expulsion of a corresponding quantity of lime, should become a carbonate of lime and magnesia. At all events, we thus remove the necessity of accounting for the origin of the magnesia discovered by analyses in the altered mass.

Another view springs from the consideration of certain other phenomena. We may assume that, whether the medium itself in which these formations took place afforded the materials, or whether they came from another place, still they did not originate, at least entirely, from the very points where the new substances were formed; but that they were conveyed to these points. We are, therefore, obliged to admit certain attractions, as the active moving agent which transported matter to such points, and also a power which prepared and formed the requisite space for the matter that was collected at these places. Examples often come before our eyes of such occurrences in pasty media, where a chemical substance is formed at certain points; but examples are not awanting where the medium was a completely solid, rigid mass, and where the same phenomenon has taken place. I shall only allude to the occurrence of mellite or honey-stone, in imbedded crystals in brown coal. Really it appears to me, that the difficulties are not much greater in understanding this phenomenon in an entirely solid medium, than in a pasty one, for the latter is certainly not much more permeable, or much more suited for the movement of fine fluids than the former, and we cannot more easily see how the shooting out of crystals can push aside the pasty mass, than how the compact mass is forced to make room for the crystals. But we cannot proceed farther, than merely thus to convey a general impression of the phenomena; this will at least preserve us from untimely and wild attempts to give explanations of occurrences, regarding which more cannot in the mean time be demanded than a simple glance at the facts as they are exhibited in nature. Thus, I do not perceive what idea can be formed, for example, respecting the occurrence of chiastolite in clay-slate; or of crystals of felspar in limestone, or in quartzose sandstone; or of so many other similar developments of crystals, if we do not adhere simply to the notions springing from the unfettered conclusions presented by nature.

From the same considerations, and generally from those resulting from the facts regarding the changes which take place in solid bodies, we have now, as I trust, to form our ideas of the formation of granite, and the other massive rocks of which we are treating. We must return to the same source when we consider the origin of gneiss, mica-slate, or generally of the crystalline slates; nay, the same leading idea will at last be made to apply to clay-slate and other crystalline rocks; for when well considered, although their origin has hitherto attracted but little attention, those last-mentioned rocks have undoubtedly not been deposited precisely in the manner in which we now find them. But still, it is chiefly the massive crystalline siliceous mountain-rocks that we have in our eye, and those namely which are found in our transition-formation. As we are now about to apply the results of our foregoing general views, it will be proper to touch also on a portion of the other rocks of the same formation, whose formation stands in certain relations of dependence to the massive rocks, or which, when viewed along with these, may throw additional light on the subject.

In the course of this discussion, I shall follow the not unusual, though sometimes certainly, the not advisable method of treating the subject in the manner as if it were supposed that the author's own theoretical views were all determined to be entirely correct; and this plan will cause us to speak of the metamorphoses that have been assumed, with as much certainty as vulcanists express themselves, when they designate granite, &c. as products of smelting. This manner of proceeding will, I hope, in the present instance, meet with so much the more approbation, when I explain that I do not wish to exalt my theory farther, than merely to regard it as a costume, a dress, with which we must clothe the new facts we have discovered, since these, in their natural nakedness, could not be introduced, or expect to be received, into the halls of science.

(*To be concluded in our next Number*).

On the Manufacture of Glass, Porcelain, and False Stones, as known to the Ancient Egyptians. With Illustrative Figures.*

Of the progress of the ancient Egyptians in many useful branches of art, we have unquestionable proofs in the monuments that remain, and from the evidence of ancient writers. The sculptures inform us that many inventions were known to them at the early periods when most other nations were still in their infancy, which, though generally ascribed to a much later epoch, are, from the facility we now have of fixing the chronology of Egyptian monuments, ascertained to be coeval with the Exodus, or the bondage of the Israelites.

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* We have to express our thanks to Mr Murray of Albemarle Street, for his kindness in facilitating the transference to our pages of the woodcuts illustrative of the Egyptian manufacture of glass. The present article is extracted from Mr Wilkinson's very valuable and elaborate work on "The Manners and Customs of the Ancient Egyptians."—Edit.

The scientific skill they possessed in architecture, is always a matter of surprise to the traveller who beholds the stupendous monuments of Egypt; whose solid masonry would have defied the ravages of time, and have remained unimpaired to the present day, had not the destructive hand of man been employed against them. The invasion of Cambyses, and the subsequent wars with the Persians; the three years' siege of Thebes by Ptolemy Lathyrus, which laid several of her buildings in ruins, and so completely reduced that ancient capital, that it was no longer worthy to be considered an Egyptian city; the inveteracy of the Christians against their Pagan predecessors, and the abhorrence of the Moslems for the monuments of the idolatrous infidels; and, lastly, the position of the temples, which presented themselves to the mason as a convenient quarry, supplying, at little labour and expense, abundance of stones for the erection of new edifices, were the baneful causes of the downfall of the Egyptian monuments. But, though great portions of the finest buildings were destroyed, sufficient remains to attest their former grandeur, and to proclaim the wonderful skill and mechanical knowledge of their founders.

At the period of the Persian invasion, Egypt was looked upon as the great school of science and the repository of all kinds of learning; but the arts had fallen from the degree of excellence to which they attained under the Augustan age of the 18th dynasty, and, though luxury and private wealth increased, taste in sculpture and architecture had long since been on the decline, and minute and highly finished details were substituted for the simple and dignified forms of an earlier period. The arts, however, continued to flourish under the succeeding dynasties, and in the reigns of Psamaticus and Amasis, the encouragement given to architecture, sculpture, and painting, seemed to promise an improvement, if not the revival of taste, and arrested for a time their downfall; but an unexpected event was destined to bring about their sudden decadence, and the Persian conquest dealt a blow from which they vainly strove to recover in the succeeding reigns of the Macedonian dynasty; for not only were the finest monuments destroyed or mutilated, statues,* works of art, and all the wealth † of the country carried off to Persia, but the artists themselves were compelled to leave their homes to follow the conquerors to their capital, and to commemorate the victories obtained over Egypt, by the authors of their own captivity and misfortunes. Thus deprived of the finest models, humbled by the lengthened occupation of the country, and losing the only persons capable of directing taste or encouraging art, Egypt, already beginning to sink, vainly endeavoured to struggle with the overwhelming current of events; and while Persia was benefited, Egyptian art received its death-blow from the invasion of Cambyses.

The Egyptians had long been renowned for mathematical science; but it was not till the power and wealth of the country were at their zenith, that full scope was given for its display in the grand style of public monuments; a fact sufficiently indicated by their increase of scale and vastness of size at that period,—the buildings of olden time being generally of much smaller dimensions than those of the advanced age of the 18th dynasty. I particularly allude to the temples and to the colossal statues erected at the latter epoch, which far exceed in their scale, and the size of the blocks themselves, the ordinary monuments of an earlier era, as may be observed in the increased proportions of the grand hall of Karnak, added by Remeses the Great, and the dimensions of the sitting colossi of Amunoph in the plain of Thebes, or that of Remeses, at the

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* Ptolemy Euergetes is said to have brought back 2500 statues, when he invaded the Persian dominions, which had been taken from Egypt by Cambyses.

† Conf. Diodor. i. 46. "The silver and gold, the abundance of ivory and precious stones carried away by the Persians," and i. 49.

Memnonium, which weighed about 886 tons, and was brought over land from the quarries at the cataracts of Syene, a distance of more than 120 miles.

Many obelisks, each of a single block of granite, had already been hewn and transported from the same quarries, as early at least as the reign of Osirtasen I., whom I suppose to have been the contemporary of Joseph; and the same mechanical skill had already existed even before that period, as is shewn from the construction of those wonderful monuments the pyramids, near Memphis, which, in the size of the blocks and their style of building, evince a degree of architectural knowledge, perhaps inferior to none possessed at a subsequent epoch. But it was not generally called forth in early times; they were then contented with monuments of an inferior scale, and their ordinary buildings were not of the same gigantic dimensions. A grand work was then seldom undertaken without an adequate motive, and the knowledge they possessed was reserved for particular and extraordinary occasions, but when riches and the love of show increased, they extended the size of their temples, and constant practice having made the means familiar to them, artisans and engineers vied with each other in hewing and transporting colossal statues, monoliths, and other ponderous monuments, which served for ornament and the display of their mechanical knowledge.

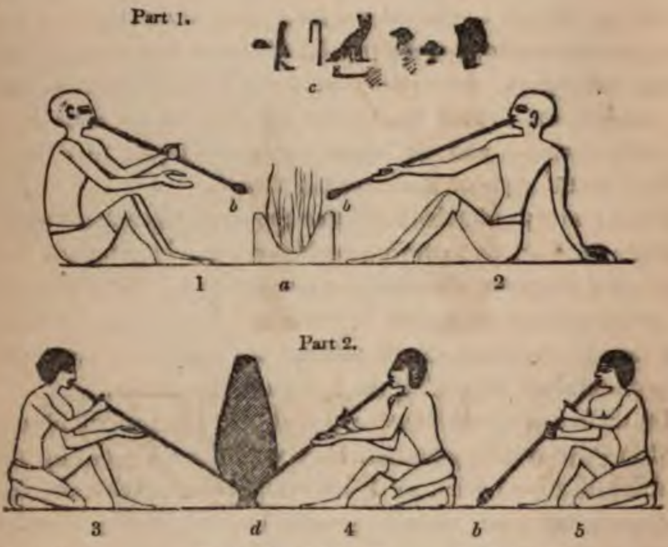
It was not in this branch of science alone that the Egyptians excelled; the wonderful skill they evinced in sculpturing or engraving hard stones is still more surprising, and we wonder at the means employed for cutting hieroglyphics, frequently to the depth of more than two inches, on basalt, on syenite, and other stones of the hardest quality. Nor were they deficient in taste,—a taste too not acquired by imitating approved models, but claiming for itself the praise of originality, and universally allowed to have been the parent of much that was afterwards perfected with such wonderful success by the most highly gifted of nations, the ancient Greeks; and no one can look upon the elegant forms of many of the Egyptian vases, the ornamental designs of their architecture, or the furniture of their rooms, without conceding to them due praise on this point, and admitting, that however whimsical some of the figures may be in sacred subjects, they often shewed considerable taste where the regulations of the priesthood and religious scruples ceased to interfere.

In their temples they were obliged to conform to rules established in the early infancy of art, which custom and prejudice had rendered sacred: the ancient style was always looked upon with the highest veneration, and it is probable that from the same feeling of respect, the formulas and diction of their books of law or religion continued the same as in early times; a custom prevalent among many people, whatever improvement language undergoes; for neither would the Turkish Moslem dare to translate the Arabic Qorán, nor the Cairene to alter it to his own dialect; and we might ourselves object to a Bible written in the style of Robertson or Hume.

Plato and Synesius both mention the stern regulations which forbade their artists to introduce innovations in religious subjects; and the more effectually to prevent this, "the profession of artist was not allowed to be exercised by common or illiterate persons, lest they should attempt any thing contrary to the laws established, regarding the figures of the deities."

In their household furniture, and the ornamental objects used in their dwelling-houses, they were not restricted by any established rules; here, as I have observed, much taste was displayed, and their vases frequently bear so strong a resemblance to those of Greece, that we might feel disposed to consider them borrowed from Greek models, did not their known antiquity forbid such a conclusion; and many have mistaken the ornamental devices attached to them and to other fancy works of Egyptian art, for the productions of Greek sculptors. Now, that we are acquainted with the dates of the Egyptian monuments, the square border and scrolls, so common on Athenian, Sicilian, Etruscan, and Græco-Italian vases, are shewn to be, from the most remote time, among the ordinary devices on cups and the ceilings of tombs at Thebes and other places, and the graceful curve of the Egyptian cornice, which, not confined to architecture, is repeated on vases and numerous articles of furniture, was evidently adopted for the same ornamental purpose by the Greeks.

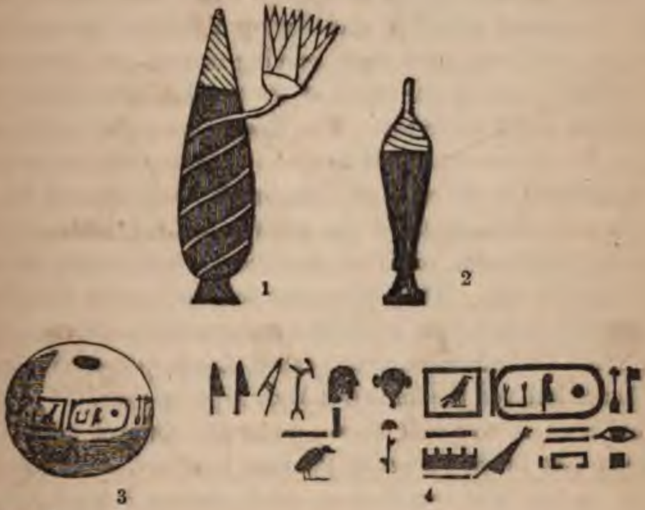
One of the most remarkable inventions of a remote era, and one with which the Egyptians appear to have been acquainted, at least as early as the reign of the first Osirtasen, upwards of 3500 years ago, is that of glass-blowing. The process is represented in the paintings of Beni Hassan, executed during the reign of that monarch, and his immediate successors; and the same is again repeated, in other parts of Egypt, in tombs of various epochs.



No. 349. Part 1. Glass-blowers. *Beni Hassan.*
2. The same. *Thebes.*

The glass at the end of the blowpipe *b b*, is coloured green.
a is the fire. *d* a glass bottle.

The form of the bottle, and the use of the blowpipe, are unequivocally indicated in those subjects; and the green hue of the fused material, taken from the fire at the point of the pipe, cannot fail to shew the intention of the artist. But if the sceptic should feel disposed to withhold his belief on the authority of a painted representation, and deny that the use of glass could be proved on such evidence, it may be well to remind him, that images of glazed pottery were common at the same period, that the vitrified substance with which they are covered is of the same quality as glass, and that, therefore, the mode of fusing, and the proper proportions of the ingredients for making glass, were already known to them; and we can positively state, that 200 years after, or about 1500 B.C., they made ornaments of glass; a bead, bearing a king's name who lived at that period, having been found at Thebes, by my friend Captain Henvey, R.N., the specific gravity of which, 2.535, is precisely the same as of crown-glass now manufactured in England.



No. 349. *a.*

Figs. 1. 2. Glass bottles represented in the sculptures of Thebes.
3. Captain Henvey's glass bead. About the real size.
4. The hieroglyphics on the bead, containing the name of a monarch who lived 1500 B.C.

Many glass bottles, and objects of various forms, have been met with in the tombs of Upper and Lower Egypt, some unquestionably of very remote antiquity, though not readily ascribed to any fixed epoch, owing to the absence of royal names, indicative of their date; and glass vases, if we may trust to the representations in the Theban paintings, are frequently shewn to have been used for holding wine, at least as early as Exodus, 1490 years before our era.

Till within a few years, prejudice forbade the belief that the ancients were acquainted with the manufacture of glass, and many persons could not be persuaded that the Romans used it, though represented in the paintings of Pompeii with the most unquestionable truth, and a pane of glass, and numerous fragments of broken bottles, had been discovered in that excavated city. The fact, however, became established, and these doubts were silenced; still it was questioned whether the invention dated before the destruction of that city; the glass was much condemned as of inferior quality; and the authority of Pliny,* previously disbelieved, was now welcomed as an old friend, and called forth to prove that glass was a late discovery of some Phœnician mariners, who having lighted a fire on the sea-shore, and supported their cooking utensils on blocks of nitre, were taught by the union of the fused substances the secret of this useful invention. The Roman naturalist had fixed no time for this event, and if he spoke of improvements in the art, introduced in the reign of Tiberius, it was presumed that, though a vitrified substance was known, its qualities were not properly understood, and that its discovery only dated about the Augustan age. They even objected, that, under the first Emperors, windows were made of a transparent stone, brought from Spain and other countries, called *Lapis specularis;* and they hence inferred the imperfect knowledge of glass.

This stone is now well known under the name of mica; it was only used in the houses of the rich, in litters, or as an ornament to the best apartments; other persons being content with linen, horn, or paper.

Such were the feeble arguments brought forward to disprove the use of glass, for vases and for ornamental purposes, among the Romans; but with much less reason did they apply to its invention in other countries; and though the Egyptians never knew the necessity, or rather the annoyance, of glass windows, under a burning sun, they were well acquainted with vases of that material; and the workmen of Thebes and Memphis, and subsequently Alexandria, were famed for the excellent qualities of glass-ware they produced, with which Rome continued to be supplied, long after Egypt became a province of the Empire. Strabo was informed by a glassmaker of Alexandria,[*] that a peculiar earth was found in Egypt, without which it was impossible to manufacture certain kinds of glass of a brilliant and valuable quality; and some vases presented by an Egyptian priest to the Emperor Hadrian,[†] were considered so curious and valuable, that they were only used on grand occasions.

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* Plin. xxxvi, c. 26.

Such, too, was the skill of the Egyptians in the manufacture of glass, and in the mode of staining it of various hues, that they counterfeited with success the amethyst and other precious stones, and even arrived at an excellence in the art which their successors have been unable to retain, and which our European workmen, in spite of their improvements in other branches of this manufacture, are still unable to imitate; for not only do the colours of some Egyptian opaque glass offer the most various devices on the exterior, distributed with the regularity of a studied design, but the same hue and the same device pass in right lines directly through the substance; so that, in whatever part it is broken, or wherever a section may chance to be made of it, the same appearance, the same colours, and the same device present themselves, without being found even to deviate from the direction of a straight line, from the external surface to the interior.

This quality of glass, of which I have seen several specimens, has been already noticed by the learned Winkelmann, who is decidedly of opinion that "the ancients carried the art of glass making to a higher degree of perfection than ourselves, though it may appear a paradox to those who have not seen their works in this material."[‡] He described two pieces of glass, found at Rome a few years before he wrote, which were of the quality above mentioned.[§] "One of them," he says, "though not quite an inch in length, and a third of an inch in breadth, exhibits on a dark and variegated ground, a bird resembling a duck in very bright and varied colours, rather in the manner of a chinese painting than a copy of nature. The outlines are bold and decided, the colours beautiful and pure, and the effect very pleasing, in consequence of the artist having alternately introduced an opaque and a transparent glass. The most delicate pencil of a miniature painter could not have traced with greater sharpness the circle of the eye-ball, or the plumage of the neck and wings, at which part this specimen has been broken. But the most surprising thing is, that the reverse exhibits the same bird, in which it is impossible to discover any difference in the smallest details; whence it may be concluded that the figure of the bird continues through its entire thickness. This picture has a granular appearance on both sides, and seems to have been formed of single pieces, like mosaic work; united with so much skill, that the most powerful magnifying glass is unable to discover their junction.

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[*] Strabo, lib. xvii.

[†] Vopiscus in Vita Saturnini, c. 8.

[‡] Winkelmann Orig. de l'Art., lib. i. 2, 19.

[§] Winkelmann, Ibid.

"From the condition of this fragment, it was at first difficult to form any idea of the process employed in its manufacture; and we should have remained entirely ignorant of it, had not the fracture shewn that filaments of the same colours, as on the surface of the glass, and throughout its whole diameter, passed from one side to the other, whence it has been concluded that the picture was composed of different cylinders of coloured glass, which being subject to a proper degree of heat, united by (partial) fusion. I cannot suppose they would have taken so much trouble, and have been contented to make a picture only the sixth of an inch thick, while, by employing longer filaments, they might have produced one many inches in thickness, without occupying any additional time in the process; it is therefore probable this was cut from a larger or thicker piece, and the number of the pictures taken from the same, depended on the length of the filaments, and the consequent thickness of the original mass.

"The other specimen, also broken, and about the size of the preceding one, is made in the same manner. It exhibits ornaments of a green, yellow, and white colour, on a blue ground, which consist in volutes, strings of beads, and flowers, ending in pyramidical points. All the details are perfectly distinct and unconfused, and yet so very minute, that the keenest eye is unable to follow the delicate lines in which the volutes terminate; the ornaments, however, are all continued without interruption, through the entire thickness of the piece."

Sometimes, when the specimens were very thin, they applied and cemented them to a small slab of stone of their own size,* which served as a support on the back, and by this means they were enabled to cut them much thinner, and consequently to increase their number.

Two of the most curious specimens I have seen of this kind of glass, have been brought to England. One is in the possession of my friend Captain Henvey, R.N., to whose kindness I am indebted for the copy of it, and of the bead before mentioned. The other was found in Egypt by Dr Hogg.

The quality and distribution of the colours in Captain Henvey's specimen are strikingly beautiful; the total size is about $1\frac{2}{10}$ inch square; and the ground is of an amethyst hue. In the centre is a device consisting of a yellow circle, surrounded by light blue, with a bright red border, and on the four sides shoot forth light blue rays, edged with white. Around this, which is isolated, runs a square ornament of bright yellow, divided into distinct parts, formed by openings in each of the sides, and at the four corners a beautiful device projects like a leaf, formed of a succession of minute lines, green, red, and white, the two last encircling the green nucleus, which meet in a common point towards the base, and terminates in almost imperceptible tenuity. The delicacy of some of the lines, is truly surprising, and not less the accuracy with which the patterns are executed; and the brilliancy of the colours is as remarkable as the harmony maintained in their disposition; an art then much more studiously attended to, and far better understood than at the present day.

The secret of making these glass ornaments is more readily explained from this specimen than any I have met with. It consists of separate squares, whose original division is readily discovered in a bright light, as well as the manner of adjusting the different parts, and of uniting them in one mass; and here and there we find that the heat applied to cement the squares has caused the colours to run between them, in consequence of partial fusion from too strong a fire. This fact, and the disposition of the separate squares, will be better understood from a reference to the plate, from which, too, some idea may be obtained of the fineness of the lines composing the devices.*

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* Mr Rogers has a specimen applied in this manner.

Not only were these various parts made at different times, and afterwards united by heat, rendered effective on their surfaces, by means of a flux applied to them, but each coloured line was at first separate, and, when adjusted in its proper place, was connected with those around it by the same process; and these, as Winkelmann very properly suggests, were cylinders or laminæ, according to the pattern proposed, which passed in direct lines through the substance or ground in which they were imbedded.

Paw, Goguet, and other antiquaries, had long ago been convinced that glass was known to the Egyptians as well as the Phœnicians, at a very remote period, and the immense emeralds mentioned by ancient authors were considered glass imitations of those precious stones; a conjecture rendered still more plausible by the experience of modern times, which shews that the most noted jewels of Christian churches are frequently formed of the same materials. Such were the colossal statue of Serapis,† in the Egyptian labyrinth, nine cubits, or thirteen feet and a half in height; an emerald presented by the King of Babylon to an Egyptian Pharaoh‡, which was four cubits or six feet long, and three cubits broad; and an obelisk§ in the temple of Jupiter, which was forty cubits, or sixty feet in height, and four cubits broad, composed of four emeralds.||

The opinions of those writers respecting the early invention of glass is now fully confirmed; and whether the first idea originated with the Phœnicians, or their neighbours the Egyptians, we have satisfactory evidence of its use 3300, or perhaps 3500 years ago.

Of the different purposes to which glass was applied by the ancients, Winkelmann gives a further account in the same chapter, where he pronounces his opinion, that "generally speaking, it was employed more frequently in ancient than in mo-

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* This reference is to a coloured plate in Mr Wilkinson's work.

† Plin. lib. xxxvii. 5, on the authority of Apion, surnamed Plistonices.

‡ Plin loc. cit. on the authority of Theophrastus.

§ Plin. loc. cit. See also Theophrastus on Stones, s. 44.

|| To have made them of glass required extraordinary skill.

dern times;" and cites, as another proof of their great skill in its manufacture, the vase preserved in the Palazzo Barberini at Rome, which, from the manner in which the layers of colour were united "had been mistaken for a real sardonyx." It is the same that is now in the British museum, and known by the name of the Portland Vase.*

That the Egyptians, at the early period of the eighteenth dynasty, were well acquainted not only with the manufacture of common glass, for beads and bottles of ordinary quality, but with the art of staining it of divers colours, is sufficiently proved by the fragments found in the tombs of Thebes; and so skilful were they in this complicated process, that they imitated the most fanciful devices, and succeeded in counterfeiting the rich hues and brilliancy of precious stones.† The green emerald, the purple amethyst, and other expensive gems, were successfully imitated; a necklace of false stones could be purchased at a Theban jeweller's to please the wearer, or deceive a stranger, by the appearance of reality; and the feelings of envy might be partially allayed, and the love of show be gratified by these specious substitutes for real jewels.

Pliny states,‡ that the emerald was more easily counterfeited than any other gem, and considers the art of imitating precious stones a far more lucrative piece of deceit than any devised by the ingenuity of man; Egypt was, as usual, the country most noted for its skill in this manufacture,§ and Strabo‖ says, "that an earth found there was the only kind which would answer for rich and variegated compositions." The emeralds mentioned by Apion and Theophrastus, which, we before observed, are supposed to have been of glass, might also be cited to shew that the art was known in the Pharaonic age, if we had not abundant and far more satisfactory proofs from specimens found in the ruins of Thebes; and we can readily believe the assertion of Pliny, that in his time they succeeded so completely in the imitation, as to render it "difficult to distinguish false from real stones."* Many in the form of beads have been met with in different parts of Egypt, and particularly at Thebes; and so far did the Egyptians carry this spirit of imitation, that even small figures, scarabæi, and objects made of ordinary porcelain were counterfeited, being composed of still cheaper materials. A figure, which was entirely of earthenware, with a glazed exterior, underwent a somewhat more complicated process than when cut out of stone, and simply covered with a vitrified coating; this last could therefore be sold at a low price: it offered all the brilliancy of the former, and its weight alone betrayed its inferiority; by which means, whatever was novel, or pleasing from its external appearance, was placed within reach of all classes; or at least the possessor had the satisfaction of appearing to partake in each fashionable novelty.

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* Some imitations of it were made by Wedgewood.

† Seneca says that Democritus first shewed the method of polishing ivory and of imitating precious stones (Epist. 90); but this was long after the art was common in Egypt. *Vide* Plin. (xxxvi. 26) "Fit et album et murrhinum aut hyacinthos sapphirosque imitatum (vitrum);" and Herodot. ii. 69, who calls them λιθινα χυτα, or melted composition of stone.

‡ Non est smaragdo alia imitabilior gemma mendacio vitri;" and "ex crystallo tingentur smaragdi, ... neque est ulla fraus vitæ lucrosior," lib. xxvii, c. 12.

§ *Vide* the Memoir of M. Boudet, "Sur l'Art de la Verrerie né en Egypte," in that valuable work the Description de l'Egypte, vol. ix. p. 213. I cannot agree with M. B. respecting the trees and the water of the Natron Lakes, p. 239, note C.

‖ Strabo, lib. xvi. p. 521, ed. Cas.

Such inventions, and successful endeavours to imitate costly ornaments by humble materials, not only shew the progress of art among the Egyptians, but strongly argue the great advancement they had made in the customs of civilized life; since it is certain, that, until society has arrived at a high degree of luxury and refinement, artificial wants of this nature are not created, and the lower classes do not yet feel the desire of imitating their wealthier superiors, in the adoption of objects dependent on taste or accidental caprice.

Glass bugles and beads were much used by the Egyptians for necklaces, and for a sort of net-work, with which they covered the wrappers and cartonage of mummies, arranged so as to form, by their varied hues, numerous devices and figures, in the manner of our bead-purses; and the ladies sometimes amused themselves by stringing them for ornamental purposes, as at the present day.

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* Plin. xxxvii. 12.

The principal use to which glass was applied by the Egyptians, besides the beads of fancy work already noticed, was for the manufacture of bottles, vases, and other utensils;* wine was frequently brought to table in a bottle, or handed to a guest in a cup† of this material, and a body was sometimes buried in a glass coffin.‡ Occasionally a granite sarcophagus was covered with a coating of vitrified matter, usually of a deep green colour, which displayed, by its transparency, the sculptures or hieroglyphic legends engraved upon the stone; a process well understood by the Egyptians, and the same they employed in many of the blue figures of pottery and stone, commonly found in their tombs; the stone, in one case, being covered with a composition capable of vitrifying, and then exposed to a certain degree of heat, until properly melted and diffused over the surface, and in the other, dipped into a mixture, which was vitrified in the same manner.

Like the Romans, they used glass for mosaic work, and pieces of various colours were employed in fancy ornaments, in the figures of deities, in sacred emblems, and in the different objects for which inlaid work was particularly adapted, the quality then used being generally of an opaque kind. In some of these vitrified compositions, the colours have a brilliancy which is truly surprising; the blues which are given by copper are vivid and beautifully clear; and one of the reds, which is probably derived from minium, has all the intenseness of *rosso antico* with the brightness of the glassy material in which it is found; thus combining the qualities of a rich enamel.

Many of the cups discovered at Thebes, present a tasteful arrangement of various hues, and evince the great skill of the

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* The lamps mentioned by Herodotus (ii. 62.), at the festival of lamps at Saïs, were probably glass. *Vide infra*, p. 122.

† In Rome the use of glass vases superseded that of gold and silver. Plin. xxxvi. 26. "Usus ad potandum argenti metalli et auri pepulit (vitrum)."

‡ Alexander the Great was said to have been buried in a glass coffin at Alexandria.

Egyptians in the manufacture of porcelain; and no one can examine similar specimens without feeling convinced of the great experience they possessed in this branch of art. The manner in which the colours are blended and arranged; the minuteness of the lines, frequently tapering off to an almost imperceptible fineness; and the varied directions of tortuous curves, traversing the substance, but strictly conforming to the pattern designed by the artist, display no ordinary skill, and shew that they were perfect masters of the means employed to produce the effect proposed.

The Egyptian porcelain should perhaps be denominated glass-porcelain, as partaking of the quality of the two, and not being altogether unlike the porcelain-glass invented by the celebrated Reaumur, who discovered, during his curious experiments on different kinds of porcelain, the method of converting glass into a substance very similar to china-ware.

The ground of Egyptian porcelain is generally of one homogeneous quality and hue, either blue or green, traversed in every direction by lines or devices of other colours—red, white, yellow, black, light or dark blue, and green, or whatever the artist chose to introduce; and these are not always confined to the surface, but frequently penetrate considerably into the ground, sometimes having passed half, at others entirely through the fused substance; in which respect they differ from the porcelain of China, where the flowers or patterns are applied to the surface, and perhaps justify the use of the term glass-porcelain, which I have adopted. In some instances, the yellows were put on after the other colours, upon the surface of the vase, which was then again subjected to a proper degree of heat; and after this, the handles, the rim, and the base, were added, and fixed by a repetition of the same process. It was not without considerable risk that these additions were made, and many vases were broken during the operation; to which Martial alludes in an epigram on the glass cups of the Egyptians.* That the Egyptians possessed considerable knowledge of chemistry and the use of metallic oxides, is evident from the nature of the colours applied to their glass and porcelain; and they were even acquainted with the influence of acids upon colour, being able, in the process of dyeing or staining cloth, to bring about certain changes in the hues,* by the same means adopted in our own cotton works, as I shall shew in describing the manufactures of the Egyptians.

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* Martial, Epig. lib. xiv. 115. Calices vitrei :—

"Adspicis ingenium Nili, quibus addere plura
Dum cupit, ah, quoties perdidit auctor opus."

It is evident that the art of cutting glass was known to the Egyptians at the most remote periods, hieroglyphics and various devices being engraved upon vases and beads, made in the time of the eighteenth dynasty; and some glass, particularly that which bears figures or ornaments in relief, was cast in a mould. Some have supposed that the method of cutting glass was unknown to the ancients, and have limited the period of its invention to the commencement of the seventeenth century of our era; when Gaspar Lehmann, at Prague, first succeeded in it, and obtained a patent from the Emperor Rodolph II.; but we may infer from the authority of Pliny, that glass-cutting was known to the ancients, and that the diamond was used for the purpose as at the present day, even if they were ignorant of the art of cutting this stone with its own dust. "Diamonds," says that author,† "are eagerly sought by lapidaries, who set them in iron handles, for they have the power of penetrating anything, however hard it may be." He also states that emeralds and other hard stones were engraved, though in early times it was "considered wrong to violate gems with any figures or devices;"‡ and the diamond was found capable of cutting those of the hardest quality, "for all gems," he observes, "may be engraved by the diamond."§

It is difficult to decide upon the precise method adopted by the Egyptians for cutting glass and hard stones; but if nothing remains to shew the process they employed, there is sufficient evidence of its effect; and their early intercourse with India may have led them to the knowledge of the diamond, and of its great utility in engraving those materials. It is also probable that emery powder, as I shall hereafter have occasion to observe, and the lapidary's wheel, were used in Egypt; and there is little doubt that the Israelites learned the art of cutting and engraving stones in that country.*

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* Plin. xxxv. 11.

† Plin. xxxvii. 4. "Expetuntur (adamantis crustae) a sculptoribus, ferroque includuntur, nullam non duritiam ex facili cavantes."

‡ Plin. xxxvii. Proem. and xxxiii. 1. He thinks the stone of Polycrates's ring was a sardonyx, xxxvii. c. 1.

§ Plin. xxxvii. 13, "Verum omnes (gemmae) adamante (scalpi possunt)."

Some glass bottles were enclosed in wicker-work, very nearly resembling what is now called by the Egyptians a *damagán*: they were generally of considerable size, holding from one to two gallons of fluid; and some of a smaller size, from six to nine inches in height, were protected by a covering made of the stalks of the papyrus or cyperus rush, like the modern bottles containing Florence oil: others, again, appear to have been partly cased in leather, sewed over them, much in the same manner as some now made for carrying liquids on a journey.

Among the many bottles found in the tombs of Thebes, none

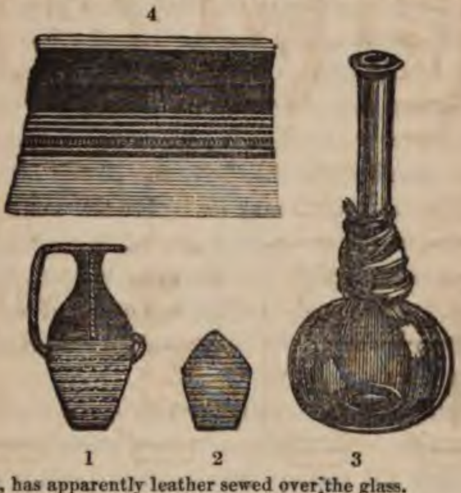

Fig. 1, has apparently leather sewed over the glass.
2, glass *damagán* enclosed in wicker work.
3, glass bottle covered with papyrus rush, like the Florence oil-flasks. *In the possession of S. Rogers, Esq.*
4, a piece of cloth with a border of a blue colour. *In my possession.*

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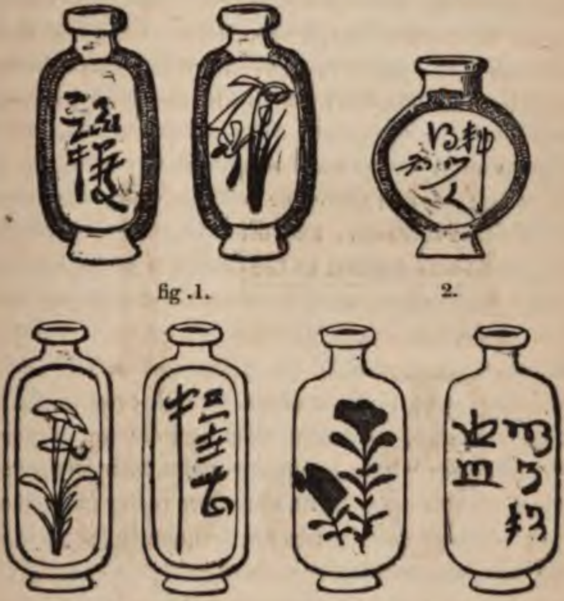
* The stones engraved by the Israelites were the "sardius, topaz, and carbuncle; the emerald, sapphire, and *diamond*; the ligure, agate, and amethyst; the beryl, onyx, and jasper." Exod. xxviii. 17, 18, 19, 20, and xxxix. 6.

have excited greater curiosity and surprise, than those of Chinese manufacture, presenting inscriptions in that language. The accidental discovery of a single bottle of this kind would naturally pass unheeded; and if we felt surprised that it should be deposited in an Egyptian sepulchre, conjecture would reasonably suggest that an accidental visitor in later times might have dropped it there, while searching for ancient treasures of a more valuable kind. But this explanation ceases to be admissible, when we find the same have been discovered in various Theban tombs. I myself have seen several, two of which I brought to England:* another is described by the learned Professor Rosellini,† and found by him "in a previously unopened tomb, of uncertain date, which," he refers, "from the



Chinese bottles found in the Egyptian tombs.
Fig. 1, in the Museum of Alnwick Castle.
2, brought by me from Thebes.
3, belonging to Mr W. Hamilton,
4, in my possession. From Thebes.

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* One is in the British Museum, the other in my possession.

† In his extensive work on the Egyptian Monuments, part 2, vol. ii. p. 337.

style of the sculptures, to a Pharaonic period, not much later than the eighteenth dynasty;" a fourth is in the museum at Jersey; another was purchased by Lord Prudhoe, at Coptos, and is now in the museum at Alnwick Castle; two others are in the possession of Mrs Bowen; and another belongs to Mr W. Hamilton. They are about two inches in height; one side presents a flower, and the other an inscription, containing, according to the valuable authority of Mr Davis (in three out of eight*), the following legend:—"The flower opens, and lo! another year."

The quality of these bottles is very inferior, and they appear to have been made before the manufacture of procelain had attained the same degree of perfection in China as in after times; they were probably brought to Egypt through India, with which country I believe the Egyptians to have traded at a very remote period, and contained some precious ingredient, whose value may be inferred from the size of the vase. It cannot be supposed that the Egyptians, who manufactured porcelain of far better quality, would have sought or imported these as articles of value; we can therefore only suppose, that they were prized for their contents; and after they were exhausted, the valueless bottle was applied to the ordinary purpose of holding the *Kohl* or Collyrium, used by women for staining their eyelids.

It has been questioned, if the Egyptians understood the art of enamelling upon gold or silver, though even in the absence of farther evidence, we might infer it from an expression in Pliny,† who says: "The Egyptians paint their silver vases, representing Anubis upon them, the silver being painted and not engraved." Small gold figures are frequently found with ornamented wings, and bodies, whose feathers, faces, or other coloured parts are composed of a vitrified composition, let into the metal; some again appear to have been really enamelled; and it is probable that the early specimens of *encaustum* were made by tooling the devices to a certain depth on bronze, and pouring a vitrified composition into the hollow space, the metal being properly heated, at the same time; and when fixed, the surface was smoothed down and polished.

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* I am happy to find that Mr Davis is preparing an account of these interesting curiosities.

† Plin. xxxiii. 9.

Both the encaustic painting in wax, and that which consisted in burning in the colours, were evidently known to the ancients, being mentioned by Pliny,* Ovid,† Martial,‡ and others; and the latter is supposed to have been on the same principle as our enamelling on gold. Pliny§ says it was uncertain to whom the invention was due; some ascribe it to Aristides, as that of perfecting the art to Praxiteles; but he supposes "it was known, long before that time, to Polygnotus, Nicanor, and Arcesilaus of Paros."

Bottles of various kinds, glass, porcelain, alabaster, and other materials were frequently exported from Egypt to other countries. The Greeks, the Etruscans, and the Romans received them as articles of luxury, which, being remarkable for their beauty, were prized as ornaments of the table; and when Egypt became a Roman province, part of the tribute annually paid to the conquerors consisted of glass vases from the manufactories of Memphis and Alexandria.

The intercourse between Egypt and Greece had been constantly kept up after the accession of Psamaticus and Amasis; and the former, the parent of the arts at that period, supplied the Greeks and some of the Syrian tribes with the manufactures they required.

The Etruscans, a commercial people, appear to have traded with Egypt, about, or a little after, the same period, and we repeatedly find small alabaster and porcelain bottles in their tombs, which have all the character of the Egyptian; and not only does the stone of the former proclaim by its quality the quarries from which it was taken, but the form and style of the workmanship leave no doubt of the bottles themselves being the productions of Egyptian artists.

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* Plin. xxxv. 11.

† Ovid, Fast. lib. viii. 275.

"———— et picta coloribus ustis
Cœlestum matrem concava puppis habet."

‡ Mart. Epig. lib. iv. ep. 39.

"Encaustus Phaëthon tabulâ depictus in hâc est;
Quid tibi vis Dipyron qui Phaëthonta facis?"

§ Plin. xxxv. 11. "Ceris pingere, ac picturam inurere quis primus excogitaverit, non constat."

It is uncertain of what stone the murrhine vases, mentioned by Pliny,* Martial, and other writers, were made; it was of various colours, beautifully blended, and even iridescent, and was obtained in greater quantity in Carmania than in any country. It was also found in Parthia and other districts of Asia, but unknown in Egypt; a fact quite consistent with the notion of its being fluor-spar, which is not met with in the valley of the Nile; and explaining the reason why the Egyptians imitated it with the composition known under the name of false murrhine, said to have been made at Thebes,† and Memphis. The description given by Pliny certainly bears a stronger resemblance to the fluor-spar, than to any other stone, and the only objection to this having been murrhine, arises from our not finding any vases, or fragments, of it; and some may still be disposed to doubt if the stone is known to which the naturalist alludes. But the fluor-spar appears to have the strongest claim; and the porcelain of Egypt, whose various colours are disposed in waving lines, as if to imitate the natural undulations of that crystallized substance, may perhaps be looked upon with reason as the false murrhine of the ancients. It is difficult to say whether the Egyptians employed glass for the purpose of making lamps or lanterns: ancient authors give us no direct information on the subject; and the paintings offer no representation which can be proved to indicate a lamp or torch, or any other kind of light.‡

Herodotus§ mentions a "fête of burning lamps," which took place at Saïs, and indeed throughout the country, at a certain period of the year, and describes the lamps used on this occasion as "small vases filled with salt and olive oil, on which the wick floated, and burned during the whole night;" but it does not appear of what materials those vases were made, though we may reasonably suppose them to have been of glass.

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* Plin. xxxvii. 2.

† Arrian, in his Periplus of the Red Sea (p. 3), mentions "λιθιας ὑαλης πλειονα γενη, και αλλης μοῤῥινης της γενομενης εν Διοσπολει." At Medeenet Haboo are numerous agatised pebbles, which were evidently brought there (the nearest known spot where they are found being Nubia), but at what period is uncertain. Were they not for some purpose connected with art? If so, it is not probable they were brought there by the Christians, though generally found upon the surface of the mounds.

‡ In the funeral processions, one person carries what seems to be a candle or torch.

§ Herodot. ii. 62.

The sculptures of Alabastron, again, represent a guard of soldiers, one of whom holds before him what resembles, and may be considered, a lantern; but here too there is great uncertainty, and neither of these are sufficient to decide the question.

An Attempt to ascertain Characters of the Botanical Alliances.

By Sir Edward Ffrench Bromhead, Bart., M. A., F.R.S. L. & E. Communicated by the Author. (Continued from vol. xxiv. p. 420).

CHARACTERS OF THE USNEACEOUS RACE.

1. Usneales.
2. Jungermanniales.
3. Lycopodiales.
4. Cupressales.
5. Betulales.
6. Rhamnales.
7. Euphorbiales.
8. Æsculales.
9. Hypericales.
10. Limoniales.
11. Fabales.
12. Violales.
13. Passiflorales.
14. Homaliales.
15. Elæagnales.
16. Acanthales.
17. Lamiales.
18. Rhinanthales.
19. Ericales.
20. Campanulales.
21. Dipsacales.
22. Myrsinales.
23. Rutales.
24. Malvales.
25. Laurales.
26. Magnoliales.
27. Menispermales.
28. Asparagales.
29. Orchidales.
30. Bromeliales.

1. Usneales.—Evascular, eductulose, cellules irregular, surface without stomata or epidermis, not herbaceous-green, without a distinct axis of vegetation, without leafy appendages. Not reproductive in water, esexual, thecæ (if present) not regularly dehiscing, sporules homonemous in germination.

2. Jungermanniales.—Gregarious, small; cellules subregular, evascular; having a distinct stem and dark fibrous roots, colour herbaceous-green; usually having crowded simple leafy appendages, amplexicaul or pseudostipulate, without stomata (exc. apophysis of Splachnum ampullaceum), acrogenous. Esexual; reproductive bodies indefinite, under two forms and ha-

ving a proper integument, contained in thecæ; calyptrate, divisions of teeth or valves binary; germinating by confervoideous geniculate threads.

Inclining to a temperate, moist, and shady habitat.

3. Lycopodiales.—Ductulose; stipe solid, more or less decumbent, having leafy appendages, acrogenous. Esexual; caulocarpous under two forms, heteronemeous in germination. Sporules indefinite and angular.

Inclining to a warm habitat.

4. Cupressales.—Arboreous or frutescent, exogenous, woody tissue with circular disks, exarticulate; multigemmate; *leaves* simple, without branched venation, usually fasciculate and acerose and rigid. Stipules 0. *Sexes* distinct, males amentaceous. *Stamens* 1 or monadelphous; *anthers* turned outwards. *Ovules* gymnospermous. *Albumen* present; *embryo* straight; *radicle* connate with the albumen.

Inclining to a temperate habitat.

5. Betulales.—Arboreous or frutescent, exarticulate, exogenous; *leaves* alternate, not dotted, not scabrous, usually deciduous; *stipules* present or leaves pinnate (Juglandaceæ), not sheathing. *Sexes* distinct, males amentaceous; *flowers* small and vernal, and often preceding the leaves. *Calyx* of the males membranous or scaly or 0. Males *apetalous*, females apetalous (exc. Juglandaceæ). *Stamens* perigynous; *anthers* bilocular. *Carpels* not more than 3 perfecting seed, connate with each other. *Albumen* 0 (exc. Liquidambar).—Inclining to a temperate habitat.

6. Rhamnales.—Arboreous or frutescent, hairs (if present) not stellate; *leaves* alternate (exc. Colletia, Retanilla). *Flowers* small, regular. *Perianth* more or less valvate or acclinate. *Sepals* 2–7, more or less connate. *Petals* (when present) not twisted. *Stamens* inserted with the petals, definite and perigynous or indefinite and hypogynous (Melanorhœa), not adelphous when there is a disk. *Disk* developed or filaments cohering at the base (some Anacardiaceæ). *Carpels* not more than 10, not more than 5 perfecting seed, forming independent cells, connate with each other or becoming solitary by abortion. *Stigma* not capitate. *Fruit* drupaceous or indehiscent (exc. Boswellia) or splitting into carpels. *Ovules* not more

than 1 or 2 to each cell, and often abortive. *Albumen* 0 (exc. some Rhamnaceæ); *radicle* towards the hilum (exc.? Spondias).

Inclining to a warm habitat.

7. Euphorbiales.—Hairs (if present) often stellate; *leaves* not dotted, simple (exc. Staphyleaceæ); *stipules* rarely absent, small or deciduous or spinescent. *Flowers* small. Odd *sepal* inferior. Divisions of the *corolla* (if any) alternating with the divisions of the calyx (if any), and not fewer. *Carpels* forming independent cells, free from the calyx, more or less connate with each other in the bud, dissepiments tending to indurate and not to be absorbed. *Seeds* 1 and erect, or 1–2 and pendulous, or axile and ascending and definite (exc. some Celastraceæ). *Albumen* (if present) enclosing the embryo; *embryo* straight or only slightly curved, axile; *radicle* to the hilum.

8. Æsculales.—*Leaves* petioled; *stipules* rarely present and then small (some Malpighiaceæ, some Sapindaceæ). *Flowers* small (exc. Æsculaceæ, Caryocar). *Sepals* in a broken series, imbricate, odd sepal inferior. *Torus* discoid. *Petals* (if present) issuing from the edge of the disk, not connate with each other, deciduous. *Stamens* hypogynous, filaments subulate or flat (Millingtonieæ), in a single or double row; *anthers* introrse, bilocular, cells parallel, not opening by pores. *Carpels* 2 or 3 or 4, forming independent cells, free from the calyx, connate with each other. *Stigmas* not sessile. *Trophosperms* central; *ovules* definite. *Albumen* 0 or extremely thin (Millingtonieæ); *radicle* next the hilum.

9. Hypericales.—*Leaves* simple; *stipules* 0. *Flowers* regular. *Sepals* 2–7, in a broken series, imbricated. *Corolla* hypogynous, of 4–10 divisions. *Stamens* hypogynous, not fewer than the corolline divisions. *Carpellary* leaves more or less turned inwards at the edges, free from the calyx, connate with each other. *Albumen* 0 or very small.

Inclining to a warm habitat.

10. Limoniales.—Arboreous or frutescent, *leaves* alternate or rarely closed up near the flowers (some Hugoniaceæ); *stipules* rarely present and then deciduous (Rhodolænaceæ) or subulate (Hugoniaceæ). *Flowers* not sessile. *Sepals* 3–5, more or less connate (exc. Rhodolænaceæ, Hugoniaceæ), not

valvate. *Petals* hypogynous, of the same number as the calycine divisions (exc. Rhodolænaceæ), alternating. *Stamens* hypogynous, not fewer than the petals, definite (exc. some Rhodolænaceæ), connected at the base by an urceolus or disk or corolline tube; *anthers* bilocular. *Carpels* 2 or more or 1 by abortion (Canelleæ, Rhodolænaceæ), forming independent cells, free from the calyx, connate with each other in the bud. *Stigmas* not sessile, *style* 1 (exc. Hugoniaceæ). *Ovules* not basilar. *Albumen* rarely absent (Trichilieæ, some Cedrelaceæ), enclosing the embryo; *embryo* straight.

Inclining to a tropical habitat.

11. FABALES.—*Leaves* petioled, usually alternate and stipulate. *Flowers* solitary or racemose or panicled. *Sepals* not exceeding 5, odd sepal inferior, sepals in a single series, more or less connected at the base, persistent. *Petals* not exceeding 5, alternating with the calycine divisions, not twisted in æstivation. *Stamens* not more than 4 times the number of petals. *Carpels* 1–5, forming independent cells, without an epigynous disk, free from the calyx (exc. the stipe occasionally), not gynobaseose, not connate with each other. *Styles* distinct. Fruit leguminous or rarely drupaceous or samaroid. *Albumen* 0 (exc. Fillæa, Cathartocarpus fistula, Cnestis).

VIOLALES.—Not arboreous (exc. Moringaceæ), not lactescent, stems round (?); *leaves* with persistent stipules or with stipulary fringes or with sheathing petioles or pinnatifid. *Calyx* of 4 or 5 divisions, odd sepal inferior or by reclination superior, imbricate, persistent (? tube at the base in Moringaceæ). *Petals* as many as the calycine divisions, alternating, not connate with each other, not valvate, more or less unguiculate or oblong (Moringaceæ). *Stamens* definite, not fewer than the petals, not more than the number of petals in a row; *anthers* or their connective or dehiscence more or less abnormal or partially abortive. *Carpels* 2–5, free from the calyx, connate with each other; edges of the carpellary leaves (if turned inwards) rarely connate at the centre (some Droseraceæ). *Placentæ* at the edges of the carpellary leaves or at the base; *ovules* many. *Fruit* a capsule, distinctly valved when the placentæ are not at the base. *Embryo* straight, erect, in the axis of albumen (if any). *Albumen* rarely absent, and then the embryo oily with very thick hypogæous cotyledons (Moringaceæ) or the seeds numerous (Parnassieæ).

13. PASSIFLORALES.—Not truly arboreous or not branched; *leaves* alternate, simple (exc. some Passifloraceæ, Turneraceæ), not sessile, petioles often glandular; *stipules* 0 (exc. some Passifloraceæ). *Cirrhi* (if any) axillary. *Flowers* not sessile. Sepals 4–7 and sometimes an inner petaloid series, more or less connate with each other, persistent (exc. some Flacourtiaceæ). *Corolla* (if any) of as many divisions as the calycine row, alternating, not valvate. Having a corolloid or filamentous *corona* (exc. Turneraceæ). *Stamens* not fewer than the calycine row; *anthers* 2-celled, bursting longitudinally. *Carpels* 3–5 (? 2–9 Flacourtiaceæ), free from the calyx (exc. Belvisiaceæ), connate with each other. *Stigmas* not sessile (exc. Caricaceæ, some Flacourtiaceæ). Carpellary leaves spread parietally open. *Placentæ* parietal, or issuing podosperms from the base (some Malesherbiaceæ); *ovules* and seeds indefinite, seeds strophiolate or arillate, or cased in pulp or mucus or in a berry. *Embryo* straight or slightly curved (Turneraceæ) the radicle not lying on the cotyledons, axile, included in fleshy albumen; *radicle* to the hilum.

14. HOMALIALES.—Arboreous or frutescent, with round branches, not lactescent; *leaves* alternate, simple, not sessile; *stipules* present (ex. some Bixaceæ, Aquilariaceæ), free, usually deciduous. *Sexes* united. *Calycine* divisions 3–15, more or less connate at the base, valvate or imbricate, persistent. *Petals* 0 or 5–15, alternating with the calycine divisions. *Stamens* not fewer than the calycine divisions, adherent to the calyx or issuing near the base; *anthers* 2-celled, bursting inwards longitudinally. *Carpels* 2–7, free from the calyx (exc. Homaliaceæ), connate with each other; edges of the carpellary leaves not turned inwards. *Stigmas* not sessile (exc. Aquilaria). *Seeds* parietal, indefinite or not more than 1 to each carpel, in pulp or arillate. *Embryo* included in the albumen (if any), or (in the solitary seeds) exalbuminous with fleshy cotyledons.

Inclining to a tropical habitat.

15. ELÆAGNALES.—Not lactescent; *leaves* (when present) not truly alternate (exc. some Proteaceæ), simple (exc. some Proteaceæ); stipules 0. *Flowers* small, usually aggregate or

umbellate. Having a *calyx* more or less petaloid, or having petals; sepals inclining to be connate when the sexes are united, a bractea or the calyx or its base persistent (exc. some Proteaceæ). *Petals* rarely present (Olacaceæ), 3–6, valvate. Fertile *stamens* 2–10, more or less adherent to the apetalous calyx (exc. Bellendena); *anthers* bilocular (exc. some Proteaceæ), dehiscing longitudinally inwards. *Carpels* 1 or 4 (Penæaceæ), carpels (if more than 1) connate with each other, free from the calyx (exc. Santalaceæ). *Stigma* on a style or subulate (Elæagnaceæ). *Ovules* not more than 4 to each carpel (exc. some Proteaceæ with stipitate ovaries); seed not more than 1 maturing for each carpel (exc. in Proteaceæ). *Albumen* (if present) fleshy, enclosing the embryo; *embryo* (if formed) straight, axile (exc. some Proteaceæ); *radicle* more or less in the direction of the hilum.

Inclining to a warm habitat.

16. ACANTHALES.—Stems round or compressed or tetragonal with perfect nodes, not lactescent; *stipules* 0. *Inflorescence* not circinate nor capitate. *Calyx* rarely obsolete (some Oleaceæ, some Acanthaceæ), herbaceous, persistent, odd calycine division superior. *Corolla* rarely absent (Fraxinus), of 4 or 5 or 8 divisions, monopetalous (exc. some Oleaceæ), more or less valvate or imbricate or labiate, following the ovary in being free from the calyx or adherent, deciduous. *Stamens* 2 or 4 or 5, or 6 connate in 2 parcels (Columelliaceæ); 2 or 4 fertile and in the latter case didynamous, adherent to the corolla, alternating with the corolline divisions; *anthers* opening longitudinally, the connective or dehiscence abnormal (exc. Jasminaceæ, Bignoniaceæ), pollen powdery. Pericarpous *disk* more or less developed (exc. Oleaceæ, Jasminaceæ, Pinguiculaceæ). *Carpels* not exceeding 4, carpels (if 2) with the midrib facing the odd sepal, connate with each other. *Style* 1 or 0 (some Oleaceæ); *stigma* bilamellate or bilobed or bifid or concave (Gesneraceæ) or undivided (some Oleaceæ, some Acanthaceæ) or quadrifid (some Pedaliaceæ). *Ovules* (if basilar or pendulous) not exceeding 2 to each carpel or (otherwise) indefinite. *Albumen* (if present) enclosing the embryo; *embryo* axile, straight (exc. some Acanthaceæ), cotyledons foliaceous.

Inclining to a warm habitat.

17. Lamiales.—*Stems* round or tetragonal with perfect nodes, not lactescent; *stipules* 0. *Inflorescence* not gyrate. *Calyx* persistent, odd calycine division superior. *Corolla* monopetalous, not plaited, more or less irregular, hypogynous, deciduous. *Stamens* not exceeding 5, 2 or 4 or rarely 5 (some Myoporaceæ) fertile, stamens (above 2) not of the same length (exc. some Verbenaceæ), adherent to the corolla, alternating with the corolline divisions. *Carpels* 2,* forming independent cells, free from the calyx, connate with each other in the bud, carpellary midrib facing the odd sepal. *Style* 1, stigma of not more than 2 divisions. *Fruit* dry, nucamentaceous. *Ovules* erect or pendulous, not more than 2 in each cell. *Embryo* straight, cotyledons foliaceous.

18. Rhinanthales.—Herbaceous or frutescent or parasitic, not lactescent, stems round or tetragonal with perfect nodes; *leaves* rarely absent (Orobanchaceæ), not compound, opposite or verticillate on the tetragonal stems; *stipules* 0. *Inflorescence* not gyrate. *Calyx* more or less divided, odd calycine division superior, persistent. *Corolla* hypogynous, monopetalous, irregular, imbricate. *Stamens* not more than 5 (exc. Disandra), 2 or 4 fertile, scarcely declinate; *anthers* opening longitudinally. *Carpels* 2, midrib facing the odd sepal (exc. on the transition to Polycarpelli), connate with each other, free from the calyx. *Styles* more or less connate. *Fruit* a capsule or rarely a berry (Leucocarpus, Teedia, Hemiphragma). *Albumen* inclosing the embryo.—Inclining to a temperate habitat.

19. Ericales.—Not truly arboreous, not lactescent; *leaves* rarely absent (Monotropaceæ), simple, entire; *stipules* 0. *Flowers* regular. *Calyx* of 4 or 5 divisions, monophyllous. *Petals* connate or subconnate with each other or the plant parasitic. *Stamens* of the same number as the corolline divisions

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* Dr Lindley considers the carpels as 2, each bilobed, because :—

1. They stand in pairs in the position of bicarpellary Scrophulariaceæ;
2. The bifid stigma;
3. Verbenaceæ split into two bilocular halves, *e. g.* Cloanthes;
4. Monsters :—When the number of carpels is multiplied in præternatural cases (*e. g.* Coleus aromaticus), the additional carpels are always in pairs, and the number of additional stigmas corresponds.

or double, stamens (when of the same number) alternating, not adelphous; *anthers* hard and dry, opening by pores or unilocular or otherwise abnormal. *Carpels* 4 or more, forming independent cells, connate with each other. *Style* 1. *Placenta* adnate to a central column. *Albumen* fleshy, embryo straight, axile; *radicle* to the hilum, centripetal.—Inclining to a mild habitat.

20. Campanulales.—Not arboreous, internodes imperfect, not regularly angled; *leaves* simple; *stipules* 0. *Flowers* axillary; sexes united. *Calyx* of 2–8 divisions, herbaceous, limb persistent. *Corolla* inserted in the throat of the calyx, monopetalous or very irregular. *Stamens* not more than the lobes of the corolla, alternating. *Anthers* bilocular, dehiscing longitudinally. *Carpels* 1 or more, connate with each other, connate with the calyx (exc. some Goodeniaceæ, Brunoniaceæ). *Style* 1, pubescent or fringed or indusiate or systaminous (Stylidiaceæ) or very short (Pongatiaceæ). *Seeds* exarillate. *Albumen* present (exc. Pongatiaceæ, Brunoniaceæ). *Embryo* straight, axile; *radicle* centripetal.

21. Dipsacales.—Not truly arboreous; *leaves* on the herbaceous stems usually marcescent; *stipules* 0. *Flowers* (if several) more or less crowded in inflorescence. *Calyx* (if any) of not more than 5 divisions, monophyllous, not petaloid, not plicate, persistent. *Corolla* of not more than 5 divisions, alternating with those of the calyx, monopetalous, not twisted. *Stamens* not more than 5, alternating with the coralline divisions; *anthers* more or less induplicate in æstivation or cohering. *Carpels* 1 or 3, only 1 perfecting seed, connate with each other, more or less connate with the calyx (exc. Globulariaceæ, Plantaginaceæ). *Fruit* not dehiscing or not regularly so; *seed* solitary if not attached to a placenta becoming finally free (some Plantaginaceæ). *Albumen* (if any) inclosing the embryo; *embryo* straight, axile; *radicle* superior where the seed is suspended from the apex or from a basilar podosperm, inferior in other cases.

Inclining to a temperate habitat.

22. Myrsinales.—*Leaves* simple, not sessile, in the woody plants usually coriaceous and persistent; *stipules* very rarely present, and then small and deciduous (Brexia). *Calyx* of 3–8

sepals or divisions, often coriaceous, persistent (exc. Pittosporaceæ). *Corolline* divisions not fewer than those of the calyx, and if more then in a double series, alternating. Petals more or less connate or conniving or valvate at the base. *Torus* not developed. *Stamens* not fewer than the calycine divisions; *anthers* bilocular, without appendages, dehiscing longitudinally inwards. *Carpels* connate with each other, dissepiments tending to absorption, free from the calyx or rarely subadherent (Samoleæ, Styraceæ). *Ovules* not parietal. *Albumen* rarely absent (Ægiceras, Brexia), not farinaceous, enclosing the embryo; *embryo* straight or nearly so, *radicle* not antitropous.

Inclining to a warm habitat.

23. Rutales.—Not lactescent. *Sexes* united or not affecting the floral envelopes. *Calyx* of 3 or 4 or 5 divisions, odd one inferior, imbricate or valvate. *Corolline* divisions alternating with those of the calyx, sooner or later larger than those of the calyx. *Stamens* inserted with the petals, definite (exc. some Ochnaceæ), not adelphous (exc. some Rutaceæ); *anthers* bilocular, introrse. *Carpels* not exceeding the number of petals (if any), distinct or appearing as distinct ribs of the ovary, gynobaseose, free from the calyx. *Stigmas* not sessile, styles tending to unite above. *Pericarp* double, the exterior fleshy or coriaceous, the interior cartilaginous or membranous. *Placenta* central; *ovules* definite (exc. some Rutaceæ).

Inclining to a warm habitat.

24. Malvales.—Branches round, hairs (if present) usually stellate; *leaves* alternate, petioled, simple; *stipules* very rarely absent, free. *Calyx* valvate in the bud or ruptile or irregular. *Petals* (when present) as many as the calycine divisions, alternating, hypogynous, twisted or convolute in the bud. *Stamens* hypogynous, a multiple of the petals or indefinite, monadelphous or within a long tubular calyx. *Carpels* forming independent cells, verticillate or connate in the bud (exc. perhaps Christiania, Malope, Sterculia, Erythropsis), free from the calyx. *Ovules* at the inner angle.

Inclining to a warm habitat.

25. Laurales.—Arboreous or frutescent or parasitic; *leaves* (if any) petioled, simple. *Stipules* 0. *Flowers* not solitary. *Calycine* divisions (if any) 3–10, sepals more or less connate,

more or less deciduous in the female flower, more or less valvate in æstivation or accumbent or formed to admit valvation. *Corolla* 0. *Stamens* definite, more or less adherent to the calyx or issuing from the torus developed. *Carpel* 1. *Stigma* peltate or sublobed or obtuse. *Fruit* not capsular. *Ovule* 1, pendulous or basilar. *Embryo* with large albumen or with large or oily cotyledons. *Albumen* (if any) ruminate, enclosing the embryo; *radicle* to the hilum.

Inclining to a tropical habitat.

26. MAGNOLIALES.—Arboreous or frutescent (exc. some Dilleniaceæ), more or less exogenous; *leaves* simple; *stipules* 0 or deciduous (Illicieæ-Magnoliaceæ). *Flowers* not sessile. Outer *Perigonium* a calyx or spatha or calycoid involucrum, perigonium divided or spathaceous. *Petals* (when present) alternating. *Stamens* many (7 or upwards); *anthers* adnate, bilocular. *Carpels* forming independent cells, more or less distinct, more or less free from the perigonium. *Fruit* (if succulent) inclining to be coadunate at maturity. *Embryo* small and included in the base of large albumen, or highly developed and exalbuminous (Calycanthaceæ); *radicle* towards the hilum or inferior.

Inclining to a tropical habitat.

27. MENISPERMALES.—Not lactescent; *leaves* not opposite, not dotted, not sessile; *Stipules* 0 (exc. pinnate Berberaceæ). *Flowers* racemose or panicled or fascicled or solitary. *Sepals* and Petals (when present) each in one or more rows, the same number in each row. *Petals* hypogynous. *Stamens* not fewer than the petals, following the germen in being free from the calyx or adherent (Eupomatia); *anthers* adnate, bilocular. *Carpels* forming independent cells; carpels distinct or connate in parcels and forming ovaries distinct in the bud; germen wholly or nearly free from the calyx. *Styles* as many as the carpels. *Ovaries* indehiscent, inclining (if succulent) to be coadunate at maturity. *Albumen* enclosing the embryo; *embryo* straight with the radicle inferior, or curved with the radicle superior.

Inclining to a warm habitat.

28. ASPARAGALES.—Not epiphytic nor parasitic, not lactescent; roots not calyptrate; *leaves* not lepidote nor equitant.

Inflorescence not spadiceous. *Perianthine* divisions not simulating distinct calyx and corolla, divisions (if petaloid) regular, not involute after flowering. *Stamens* 3–6; *anthers* bilocular. *Carpels* 2 or 3 or 4, free from the calyx (exc. in some unisexual flowers, Dioscoreaceæ). Fruit not drupaceous nor nucamentose. Valves (if any) carrying the dissepiments, hilum not rostellate. *Albumen* present, enclosing the embryo; *embryo* not trochlear; *radicle* to the hilum.

Inclining to a temperate habitat.

29. ORCHIDALES.—Not annuals, roots not calyptrate; *leaves* simple, entire, more or less sheathing. *Inflorescence* not spadiceous, flowers not minute. *Perianth* in six divisions, not glumaceous; the odd exterior division inferior, or by a twisting superior; three or more of the divisions petaloid, or all petaloid if in a single row. *Stamens* not more than six. *Carpels* not more than three, connate with each other, connate with the perianth. *Embryo* (if formed) included in albumen, not macropodal.

Inclining to a warm habitat.

30. BROMELIALES.—Herbaceous or rarely frutescent (some Bromeliaceæ); *leaves* (if any) undivided (exc. some Taccaceæ). *Perianth* present, divisions in one or two rows, each row in the male flowers of three divisions, outer row more or less connate, inner row petaloid. *Stamens* 3 or 6 or more, (if 3) facing the inner perianth, adherent to the perianth, free from each other; *anthers* two-celled, turned inwards. *Carpels* 3, connate with each other, connate with the perianth (exc. Tillandsieæ, Wachendorfieæ). *Stigmas* not sessile, styles connate. *Ovules* not basilar, one or two to each carpel or indefinite. *Albumen* present. [The RHIZANTHS are not included.]

References to the Table.

(1) These are Coniothalami, Idiothalami, Hymenothalami, Gasterothalami (exc. ? Sphaerophoreae).

(2) Asteraceae are Asteraceae-cichoraceae-mutisiaceae-cynaraceae—followed by (Xeranthemeae)-calendulaceae-arctotideae.

Asparagaceae are Convallarinae-parideae-asparageae-aloinae-anthericeae.
Celastraceae are Celastraceae-staphyleaceae-hippocrateae-trigonieae.
Hemerocallaceae are †Scilleae-hemerocallideae-tulipeae.
Oleaceae are Oleaceae-jasminaceae.
Rhinanthaceae are (Hemimerideae-antirrhineae)-gerardieae-†rhinantheae.
Smilaceae are Smilaceae-dioscoreaceae-†roxburghiaceae.

THE USNEACEOUS RACE.

††FUNGI,...Byssaceae, calyciaceae,[1] graphidaceae,[1] USNEACEAE,[1] endocarpaceae,[1]

Ricciaceae, marchantiaceae, JUNGERMANNIACEAE, andraeaceae, bryaceae,
Salviniaceae, marsileaceae, isoëtaceae,[6] †LYCOPODIACEAE, lepidodendraceae,[2]
(Salisburiaceae[3]), †taxaceae, CUPRESSACEÆ, †pinaceae, araucariaceae,

Liquidambraceae, salicaceae, BETULACEAE, carpineae[2]-†corylaceae, juglandaceae,
Anacardiaceae-spondieae, burseraceae, chailletiaceae, nitrariaceae-(neuradeae), RHAMNACEAE,
[Coriariaceae, (†)EUPHORBIACEAE, empetraceae-stackhousieae, celastraceae, erythroxylaceae,

Malpighiaceae, (†)aceraceae, ÆSCULACEAE, millingtonieae-sapindaceae, caryocaraceae,[1]
†Clusiaceae, marcgraaviaceae, HYPERICACEAE, ochranthaceae-carpodonteae, †camelliaceae,
Rhodolaenaceae, humiriaceae, (hugoniaceae)-(canelleae), meliaceae-cedreleae, LIMONIACEAE,

Amyridaceae, †connaraceae, mimoseae-detarieae, swartzieae-†FABACEAE, geoffroyeae-caesalpinieae,

Moringaceae, [wormskioldiae[2]-†frankeniaceae-sauvagesiae, (†)parnassieæ,[7] droseraceæ, †VIOLACEÆ,]
†PASSIFLORACEAE-malesherbieae, turneraceae, caricaceae, belvisiaceae, patrisieae-†flacourtiaceae,
Bixaceae, pangiaceae, samydaceae, HOMALIACEAE, aquilariaceae,

†Daphnaceae-penaeaceae, †proteaceae, ELÆAGNACEÆ, nysseae[5]-†santalaceae-anthoboleae, olacaceæ,

Oleaceae, columellieae-gesneraceae, pinguiculaceae, ACANTHACEAE, cyrtandreae-bignoniaceae-pedalieae,
Stilbaceae, selaginaceae, myoporaceae, verbenaceae, LAMIACEAE-ocimoideae,
Buddleieae-buchnereae, †gratioleae, (†veroniceae), †RHINANTHACEAE,[9] orobanchaceae,

Monotropaceae-(†)pyrolaceae, [†ERICACEAE, epacridaceae, andromedeae, vacciniaceae,]
CAMPANULACEAE-lobelieae, stylidiaceae, pongatiaceae, goodeniaceae-scaevoleae, brunoniaceae,
Asteraceae,[9] valerianaceae, calyceraceae, DIPSACACEAE-globularieae, †plantaginaceae,

†Primulaceae-MYRSINACEAE, achrasaceae, (†)styraceae-diospyraceae, ilicaceae-brexieae, pittosporaceae,
†Zygophyllaceae, †RUTACEAE, †xanthoxylaceae, simarubaceae, ochnaceae,
Dipterocarpaceae, elaeocarpeae-tiliaceae, byttnerieae-dombeyae-bombaceae, MALVACEAE, sterculiaceae,

Myristicaceae, hernandiaceae, illigeraceae, cassythaceae, LAURACEAE,
Atherospermaceae, monimiaceae, calycanthaceae, illicieae-MAGNOLIACEAE, †dilleniaceae,
Schizandraceae, †anonaceae, †berberaceae-(nandineae), lardizabaleae, MENISPERMACEAE,

Smilaceae,[9] †ASPARAGACEAE,[9] (†)juncaceae-gilliesieae, †hemerocallaceae,[9] (†)melanthiaceae,
Iridaceae, apostasiaceae, ORCHIDACEAE-vanilleae, zingiberaceae-maranteae, musaceae,
†Amaryllidaceae, †BROMELIACEAE, †haemodoraceae, burmanniaceae, (taccaceae),...†RHIZANTHEAE.

Lat. 50° 40′ N.; Long. 7° 12′ E. from Greenwich.

	Hour	Barometer Red. to 32°.	Attached Therm.		Therm. Fahrenheit.		Rain Gauge.	Wind.		Appearance of the Sky and Remarks.
		Eng. Inch.	Cent.	Fahr.	Dry.	Moist.	Inch.	Direction.	Force.	
June 21, 1837.	6 A.M.	28,983	19.5	67.1	59.8	54.5	0	E.	light	Generally clear; cirrocumuli stretching from S. in various directions, particularly in E., cirrostrati in west.
	7 ...	28,986	19.6	67.3	60.8	56.4	0	E.	light	Many cirrostrati in horizon; bands of cirrocumuli and cirri stretching from S. to N.
	8 ...	28,989	20.0	68.0	64.0	58.5	0	E.	lighter	Horizon as before, cirri generally diffused, especially in E.
	9 ...	28,998	19.7	67.5	66.6	60.0	0	SE.	scarcely percep.	Generally clearer; cirri in the east; cirrocumuli west of the zenith.
	10 ...	28,988	21.0	69.8	69.2	61.2	0		insensible	Generally clearer; floating cumuli moving from S.
	11 ...	28,995	22.0	71.6	71.5	62.0	0		insensible	Clear in zenith, great quantity of cumuli in E., cirri and cirrocumuli in W., moving as before.
	12 ...	28,994	23.7	74.7	71.7	63.2	0	SE.	feeble	Cumuli pretty generally diffused, especially in zenith, some cirri in W.
	1 P.M.	29,003	24.5	76.1	75.5	64.2	0	S.	light	Clear in S.; cumuli in E. and N., with cirrocumuli in W.
	2 ...	28,992	26.7	80.1	80.0	67.2	0	E.	light	Cirri in the zenith; cumuli in all quarters of the horizon.
	3 ...	28,993	26.5	79.7	78.7	66.3	0		insensible	Clear in zenith; cumuli in W.
	4 ...	29,002	25.5	77.9	73.5	63.5	0	E.	light	Clear, with a few cirri in zenith; cumuli stretching from S. to NW.
	5 ...	29,006	25.5	77.9	72.5	62.0	0	E.	very feeble	Clear in zenith and W.; great quantity of cirri in E.
	6 ...	29,003	25.2	77.4	74.5	63.5	0		insensible	Cirrocumuli in N. and E.; clearer in W., with large black mass of cumuli.
	7 ...	29,012	25.0	77.0	70.0	61.0	0	SE.	scarcely percep.	Cirrocumuli pretty generally diffused; cumulo-stratous clouds in NW., and cirrostratous in E.
	8 ...	29,009	22.5	72.5	63.8	63.0	0	W.	strong and gusty	General appearance of sky as before; obscurer and more thickly clouded.
	9 ...	29,011	23.5	74.3	63.4	62.2	0	NW.	gusty	Sky covered with clouds, particularly heavy in S.
	10 ...	29,098	22.6	72.7	61.8	61.1	0	NNW.	pretty strong	Uniformly clouded.
	11 ...	29,093	22.8	73.0	60.6	60.0	0	NNW.	strong	Uniformly cloudy.
	12 ...	29,098	22.6	72.7	60.6	60.2	0	S.	moderate	The sky still generally clouded, but the horizon clearer in several places.
June 22, 1837.	1 A.M.	29,107	22.4	72.4	60.2	60.0	0	NW.	moderate	Nearly uniformly overcast.
	2 ...	29,109	22.0	71.6	58.5	58.3	0	NW	strong	As before: Wind more violent.
	3 ...	29,123	21.5	70.7	57.7	57.2	0	NW.	gusty	Clouds generally diffused: Outline of the Castle obscured by mist.
	4 ...	29,104	21.0	69.8	57.0	57.1	0	NW.	moderate	Very thick heavy mist enveloping the Hill, and slightly wetting the dress.
	5 ...	29,108	20.3	68.5	57.5	57.5	0	WNW.	light	Mist continuing, but slightly clearer.
	6 ...	29,125	20.0	68.0	56.5	56.5	0.002	W.	strong	Thick mist with small rain.
	7 ...	29,142	19.7	67.5	55.5	55.7	0.004	W.	rather strong	Impenetrable white mist with small rain.
	8 ...	29,144	19.5	67.1	55.8	56.0	0.004	NNW.	moderate	Continued thick mist.
	9 ...	29,157	19.2	66.6	56.8	57.2	0.002	N.	feeble	As before.
	10 ...	29,161	19.2	66.6	58.0	58.3	a trace	N.	nearly insensib.	Mist less thick. Castle faintly appears.
	11 ...	29,187	19.6	67.3	60.0	59.4	0	N.	nearly insensib.	Greatly clearer. Sky still covered by clouds of dull grey.
	12 ...	29,186	19.7	67.5	60.3	60.0	0	W.	feeble gusts	Sky covered by greyish clouds. Fog on low ground, and on the head of Lowenburg.
	1 P.M.	29,183	18.8	65.8	63.0	61.3	0	NW.	feeble	Clouds beginning to break. Atmosphere a little clearer.
	2 ...	29,179	19.5	67.1	64.5	62.0	0	N.	moderate	Clearer. Large masses of cumuli spread over the sky.
	3 ...	29,186	20.5	68.9	66.0	62.0	0	NW.	light	Sky pretty generally covered with thick cumuli; faint gleams of sunshine.
	4 ...	29,184	21.0	69.8	67.5	63.5	0	N.	light	Clear in zenith, and to N., S., and E. cloudy. Cumuli in W.
	5 ...	29,187	21.2	70.2	68.3	63.8	0	N.	feeble	Clear towards north-west; cirrocumuli abundant.
	6 ...	29,187	22.0	71.6	68.0	63.0	0	N.	light	Much clearer; sunshine; cirrocumuli scattered about.
Means,...		29,083	22.77	71.2	64.58	60.51				

THE USNEACEOUS RACE.

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Ricciaceae, marchantiaceae, JUNGERMANNIACEAE, andraeaceae, bryaceae,
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(Salisburiaceae[5]), †taxaceae, CUPRESSACEÆ, †pinaceae, araucariaceae,

Liquidambraceae, salicaceae, BETULACEAE, carpineae[2]-†corylaceae, juglandaceae,
Anacardiaceae-spondieae, burseraceae, chailletiaceae, nitrariaceae-(neuradeae), RHAMNACEAE,
[Coriariaceae, (†)EUPHORBIACEAE, empetraceae-stackhousieae, celastraceae, erythroxylaceae,

Malpighiaceae, (†)aceraceae, ÆSCULACEAE, millingtonieae-sapindaceae, caryocaraceae,[7]
†Clusiaceae, marcgraaviaceae, HYPERICACEAE, ochranthaceae-carpodonteae, †camelliaceae,
Rhodolaenaceae, humiriaceae, (hugoniaceae)-(canelleae), meliaceae-cedreleae, LIMONIACEAE,

Amyridaceae, †connaraceae, mimoseae-detarieae, swartzieae-†FABACEAE, geoffroyeae-caesalpinieae,

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Bixaceae, pangiaceae, samydaceae, HOMALIACEAE, aquilariaceae,

†Daphnaceae-penaeaceae, †proteaceae, ELÆAGNACEÆ, nysseae[5]-†santalaceae-anthoboleae, olacaceæ,

Oleaceae, columellieae-gesneraceae, pinguiculaceae, ACANTHACEAE, cyrtandreae-bignoniaceae-pedalieae,
Stilbaceae, selaginaceae, myoporaceae, verbenaceae, LAMIACEAE-ocimoideae,
Buddleieae-buchnereae, †gratioleae, (†veroniceae), †RHINANTHACEAE,[9] orobanchaceae,

Monotropaceae-(†)pyrolaceae, [†ERICACEAE, epacridaceae, andromedeae, vacciniaceae,]
CAMPANULACEAE-lobelieae, stylidiaceae, pongatiaceae, goodeniaceae-scaevoleae, brunoniaceae,
Asteraceae,[9] valerianaceae, calyceraceae, DIPSACACEAE-globularieae, †plantaginaceae,

†Primulaceae-MYRSINACEAE, achrasaceae, (†)styraceae-diospyraceae, ilicaceae-brexieae, pittosporaceae,
†Zygophyllaceae, †RUTACEAE, †xanthoxylaceae, simarubaceae, ochnaceae,
Dipterocarpaceae, elaeocarpeae-tiliaceae, byttnerieae-dombeyae-bombaceae, MALVACEAE, sterculiaceae,

Myristicaceae, hernandiaceae, illigeraceae, cassythaceae, LAURACEAE,
Atherospermaceae, monimiaceae, calycanthaceae, illicieae-MAGNOLIACEAE, †dilleniaceae,
Schizandraceae, †anonaceae, †berberaceae-(nandineae), lardizabaleae, MENISPERMACEAE,

Smilaceae,[9] †ASPARAGACEAE,[9] (†)juncaceae-gilliesieae, †hemerocallaceae,[9] (†)melanthiaceae,
Iridaceae, apostasiaceae, ORCHIDACEAE-vanilleae, zingiberaceae-maranteae, musaceae,
†Amaryllidaceae, †BROMELIACEAE, †haemodoraceae, burmanniaceae, (taccaceae),...†RHIZANTHEAE.

Quarterly **Meteorological** *Observations taken on the 21st and 22d December 1837 by a Committee of the Physico-Mathematical Society of Edinburgh.*

	Hour.	Barometer corrected.	Thermometers.			Rain-gauge.	Wind.		General Remarks.
			Attached.	External.	Moistened.		Direction.	Force.	
			°	°	°	Inches.			
December 21, 1837,	6 A. M.	29,771	56.8	38.7	38.0	...	SSE.	Brisk	Sky cloudy along the horizon; clear in E.
	7 ...	29,727	58.0	37.0	36.0	...	S.	Light	Clear in zenith; cloudy near the horizon.
	8 ...	29,814	56.6	38.1	36.7	...	SSE.	do.	Clouds rising into the zenith.
	9 ...	29,883	53.5	38.7	37.5	...	SW.	Brisk	General mistiness; clouds seem breaking up.
	10 ...	29,983	55.7	39.5	36.5	...	S.	do.	Clearer in zenith; light clouds still remaining.
	11 ...	29,886	56.5	39.5	36.5	...	S.	Light	Mist gone; light clouds over the whole sky.
	12 ...	29,886	56.2	40.0	37.5	...	SW.	do.	Slight mist; cumulo-strati W.; mass of clouds E.
	1 P. M.	29,890	55.5	39.7	36.5	...	S.	do.	Still cloudy; clouds higher; mist gone.
	2 ...	29,865	56.0	40.5	37.0	...	S.	do.	Heavy bank of clouds in N.; lighter in SW.
	3 ...	29,875	53.9	39.9	37.1	...	S. by W.	Brisk	do. do.
	4 ...	29,880	56.8	41.5	38.0	...	S.	Strong	Generally cloudy.
	5 ...	29,857	56.3	40.5	38.4	0.03	S.	Violent	Cloudy; heavy rain.
	6 ...	29,834	55.5	41.1	38.8	0.007	S.	Strong	do. fair.
	7 ...	29,866	56.2	43.2	40.3	0.005	S. by W.	do.	do. slight rain.
	8 ...	29,864	56.0	44.4	42.0	...	SW.	do.	do. fair.
	9 ...	29,750	56.7	*		...	do.	Violent	do. no rain.
	10 ...	29,687	56.8			...	do.	do.	do. do.
	11 ...	29,664	56.8			...	SSW.	Strong	do. rain.
	12 ...	29,659	55.5	47.8	45.3	...	do.	do.	do. fair; clear along the N.
Dec. 22,	1 A. M.	29,658	58.6	48.5	45.8	...	S.	Light	do. do.
	2 ...	29,660	59.9	48.2	46.3	0.005	W.	do.	Dark, and drizzling rain.
	3 ...	29,674	60.5	48.5	46.0	...	SW.	do.	Cloudy: a few drops of rain at intervals.
	4 ...	29.661	59.8	48.2	46.8	0.01	S.	do.	Cloudy, and raining.
	5 ...	29,637	59.7	48.7	46.8	0.005	SW.	do.	Dark, cloudy and raining.
	6 ..	29,595	60.0	48.5	46.8	0.01	do.	do.	A little lighter; still heavy rain.
	Means,...	29,780	57.0	42.7	40.5	0.072			

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* Owing to the violence of the wind, the thermometers were broken. They were replaced by 12 o'clock

Quarterly Meteorological Observations, taken on the 21st and 22d March 1838, *by a Committee of the Physico-Mathematical Society of Edinburgh.*

	Hour.	Barometer corrected.	Thermometers.			Rain-gauge.	Wind.		General Remarks.
			Attached.	External.	Moistened.		Direction.	Force.	
			°	°	°	Inches.			
March 21, 1838.	6 A. M.	28,898	51.3	35.9	34.8	...	N.	Strong	Sleet, and cloudy.
	7 ...	28,925	48.5	36.2	34.6	...	N.	do.	do.
	8 ...	29,019	52.8	37.2	34.7	0.03	N.	do.	Cloudy.
	9 ...	29,057	53.3	37.2	34.8	...	NW.	Brisk	do.
	10 ...	29,109	50.5	37.0	34.5	...	NNE.	Strong	Cumulo-strati over the whole sky; lighter in the N.
	11 ...	29,112	52.5	37,7	33.7	...	do.	Gusty	Still cloudy; lighter towards the north.
	12 ...	29,132	53.0	38.7	34.5	...	do.	Brisk	Clouds driving along; lighter in zenith.
	1 P. M.	29,132	53.7	39.1	34.8	...	do.	Gusty	Still cloudy.
	2 ...	29,158	53.4	39.2	35.0	...	N.	Strong	Cloudy; dark cumulo-strati in the S.
	3 ...	29,183	52.0	39.2	34.8	...	N.	do.	do. clear towards the N.
	4 ...	29,233	51.5	38.9	34.6	...	N.	do.	Cloudy.
	5 ...	29,234	52.3	38.3	34.0	...	NNE.	do.	do. lighter in the N.
	6 ...	29,231	51.3	37.7	34.0	...	N.	do.	do.
	7 ...	29,277	53.5	37.3	34.1	...	N. by E.	Brisk	do. mist near horizon.
	8 ...	29,293	52.5	37.1	34.2	...	do.	do.	Generally cloudy.
	9 ...	29,339	50.5	37.0	33.7	...	N.	do.	Dark clouds, especially towards SW.
	10 ...	29,358	50.3	36.4	33.5	...	NNE.	Strong	Clouds separating; one or two stars visible.
	11 ...	29,381	52.7	36.2	33.5	...	N. by E.	do.	Lighter; clouds unbroken.
	12 ...	29,394	54.5	36.2	33.2	...	NE.	do.	Much clearer; stars in zenith seen.
March 22,	1 A. M.	29,436	55.0	36.0	33.2	...	NNE.	Brisk	Many dark clouds.
	2 ...	29,498	55.5	35.7	33.0	...	N. by E.	Lighter	Clearer; stars seen through the clouds.
	3 ...	29,506	55.0	36.2	33.5	...	N.	Brisk	Cloudy in horizon; stars seen in zenith.
	4 ...	29,539	53.5	35.9	33.8	...	N.	Light	Thick mist, and dark clouds.
	5 ...	29,546	53.5	35.7	33.8	...	N. by E.	Strong	Light grey cumuli over the whole sky.
	6 ...	29,551	53.4	35.7	33.0	...	N.	do.	Generally cloudy; clearer along north horizon.
	Means,...	29,267	52.5	37.4	34.0	0.03	NNE.		

NOTE to the Quarterly Meteorological Observations from December 1836 *to March* 1838, *published in the Edinburgh Philosophical Journal for October* 1837 *and July* 1838.

The barometer used in December 1836, was a cistern one by Robinson of London. On all the other occasions a syphon one, on Bunten's construction, made by Adie of Edinburgh, was employed. It was placed in a closet adjoining a room in which there was a fire. In December 1836 and March 1837, the external thermometer was placed outside a window having a northern aspect, and in June and December 1837, and March 1838, on the roof under shelter. In December 1836 and March 1837, the moisture was ascertained by Leslie's hygrometer; for which a moistened bulb thermometer was substituted in the subsequent observations. The place of observation is about 160 feet above the sea.

The observations for September 1837, were taken at a height about 15 feet above the other station. The instruments were the same. The barometer was placed at a window having a southern aspect, but removed during the day to one having a northern aspect. The thermometers were suspended in the shade near the barometer. From the indications of the external thermometer, 0°.5 must be substracted from those of the moistened thermometer 0°.1.

The barometer in December 1836, was not corrected for capillarity. With that and other exceptions mentioned, all necessary corrections have been applied.

At the June observations on the Drachenfels, at which one of the members of the Committee assisted, the barometer used was by Bunten of Paris, and carefully protected from the sun. The thermometers were placed near the barometer. Ths error of the thermometers was ascertained a few days afterwards, when the moistened thermometer stood 0°.2 above, and the other 0°.2 below the freezing point. The variation in the height of the barometer at 10 P. M. June 21, was partly occasioned by the instrument having been removed and replaced, which seems to have removed some previously existing adhesion.

Since September 1837, Sir J. Herschel has recommended that the observations should not be prolonged beyond twenty-five hours, as no additional benefit was found to result from the longer period. This suggestion has been complied with in the subsequent observations.

Address to the Geological Society, at the Anniversary on the 16*th of February* 1838. By the Rev. WILLIAM WHEWELL, M. A., F. R. S., President of the Society.

GENTLEMEN,

You have heard in the reports just read, statements which shew that the society is in a state of healthy progress both in respect to its numbers and its funds. The total number of Fellows of the Society, exclusive of the Honorary and Foreign Members, at the close of the year 1836, was 709. At the close of the last year it was 738, the increase being 29, after deducting 18 members deceased or resigned.

A part of the Transactions has recently been published, which is worthy of its predecessors in the interest of its matter, and which is not inferior to them in its appearance and illustrations. I believe it will be found that improvements have been introduced, especially in the colouring of the maps.

Our collections have also gone on increasing, and have, as in previous years, derived great additional value from the labour and knowledge bestowed upon them by our excellent curator. But your Council has found itself compelled to attend to the great, and I may say intolerable amount of labour which has fallen upon Mr Lonsdale, and certain alterations in the Society's arrangements, directed to the object of remedying this evil, are now in progress or in contemplation. When they are completed I shall have the satisfaction of announcing them to the Society.

The Council have awarded the Wollaston Medal, as you have already been informed, to Mr Richard Owen, for his general services to Fossil Zoology, and especially for his labours employed upon the fossil mammalia collected by Mr Darwin in the voyage of Captain Fitz Roy. I need not remind you, Gentlemen, how close are the ties which connect the study of living and of fossil animals; how much light the progress of comparative anatomy throws upon the interpretation of geological characters; and what important steps in our knowledge of the past condition of the earth are restorations of the animal forms which peopled its surface in former times, but have long vanished away. Since the immortal Cuvier breathed into our science a new principle of life, the value of such researches has ever been duly appreciated;

and the award of the Wollaston Medal last year is an evidence how gladly your Council take that method of congratulating the successful cultivators of such studies. I am sure that all who are acquainted with Mr Owen's labours will rejoice that we have in this manner marked our sense of his success. His earlier researches, those for instance on the Nautilus, have been of exceeding use and interest to geologists. And the first part of his description of the fossil mammalia, collected by Mr Darwin in South America, contains matters of the most striking novelty, interest, and importance. We have there the restoration, performed with a consummate skill, such as fitly marks the worthy successor of Hunter and the disciple of Cuvier, of two animals, not only of new genera, but occupying places in the series of animal forms, which are peculiarly instructive. For the one, the Toxodon, connects the Rodentia with the Pachydermata by manifest links, and with the Cetacea by more remote resemblances; and thus contributes to the completion of the zoological scale just in the parts where it is weakest and most imperfect: while the other animal, the Macrauchenia, the determination of which is considered by anatomists as an admirable example of the solution of such a problem, appears to be exactly intermediate between the horse and the camel. But this creature is also interesting in another way, since it closely resembles, although on a gigantic scale, an animal still existing in that country, and peculiar to it, the Llama. Thus, in this, as in some other instances, the types of animal forms which distinguish a certain region on the earth's surface, are clearly reflected to our eyes as we gaze into the past ages of the earth's history, while they are magnified so as to assume what almost appear supernatural dimensions. The Llama, the Capybara, and the Armadillo of South America are seen in colossal forms in the Macrauchenia, the Toxodon, and the Megatherium. I will not omit this occasion of stating, that the profound and enlarged speculations on the diffusion, preservation, and extinction of races of animals to which Mr Darwin has been led by the remains which he has brought home, give great additional value to the treasures which he has collected, and make it proper to offer our congratulations to him, along with Mr Owen, on the splendid results to which his expedition has led, and is likely to lead. Mr Owen and Mr Darwin are engaged in the restoration of other animals from the South American remains in their possession, and I am able to announce that two or three other new genera have already been detected. I am sure I am conveying your feeling, Gentlemen, as well as my own, when I express a cordial hope that these two naturalists, so fitted by their endowments and character to advance the progress of science, may long go on achieving new triumphs; and may have the satisfaction—higher even than that which they derive from the honours we so willingly bestow—of finding the great principles which it is given to them to wield, becoming every year more powerful instruments of discovery; and of seeing, as they pursue their researches, light thrown upon the darkest and widest of the vast problems which they have proposed to themselves.

I will now say a few words concerning a few of the most conspicuous of the names which have been obliterated by death from our list during the year.

Among the members of our body, whom we have lost, there is one whom we cannot but mention with more than common emotion, endeared as he was to many of us by private friendship, and admired by all for his talents, his knowledge, and his services. Dr Edward Turner, Professor of Chemistry in the London University, filled the office of our Secretary for five years, and subsequently was two years Vice-President, which situation he held at the time of his death in February 1837. Several of you may remember, Gentlemen, that our last anniversary meeting was in some measure clouded by the recollection of this then recent calamity; and that many of the Fellows of the Society, on that occasion, expressed their intention of testifying their respect and regard for the departed by attending his funeral. Of Dr Turner's private virtues, and of the charm of his society, I must not here speak. I will not allow myself to dwell upon the admirable clearness and precision of his thoughts as expressed in conversation,—upon the delightful openness and candour of his character,—upon the kind and gentle cheerfulness of his demeanour, the genuine fruit of a deep habitual religious feeling. But I may take this occasion to say, that in him chemistry suffered a loss, not only great,—for that all would at once say,—but much greater and more difficult to repair than may at first sight appear. Dr Turner entertained a conviction (I am stating the result of many interesting conversations which I have held with him) that the time was come when the chemist could not hope to follow out the fortunes of his science, and to read in her discoveries their full meaning, without being acquainted with the language, and master of the resources of mathematics. Acting upon this enlightened view, he did not hesitate to encounter the great labour and exertion of a course of study in the higher mathematics; and he succeeded entirely in making himself a good mathematician. And he was one of the very few who, in our country, labour at a branch of chemistry which is of the highest importance to us as geologists; but which,—we may suppose from its laborious and intricate nature,—appears to repel our most active chemists; I mean that portion of chemistry which is connected with mineralogy.

Yet this department is, in truth, more inviting than it may at first appear. No doubt in it clear mathematical conceptions are necessary, and perhaps some little training in mathematics; but there is good promise that the labour which this line of investigation demands will be rewarded. I am fully persuaded that there is no portion of the frontier line of our knowledge of which we can so certainly say, "Here we are on the brink of great discoveries." Had Dr Turner been spared to us some years longer, I know no one who was more likely to have had a principal share in such discoveries. Two papers of his, in the Philosophical Transactions,* show that he was able to deal with the atomic theory in a mode which combines the resources of the skilful analytical chemist with the rigour of the mathematical reasoner; a combination which the right prosecution of that theory requires, but which has not always been found in its cultivators.

Dr Turner lectured on chemistry at the London University from its first foundation in 1828; he was there surrounded by students, whose affection he gained by his kindness, as well as their admiration by the clearness of his teaching. He also gave a course of lectures on geology, in conjunction with

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* On the Composition of Chloride of Barium, 1829; Researches on Atomic Weights, 1833.

Dr Grant and Mr Lindley, each of those gentlemen taking a division of the subject with which he was most familiar. Dr Turner was snatched from science at the early age of thirty-nine, having been born in the island of Jamaica in 1796. He studied anatomy at Edinburgh, and chemistry at Göttingen, under the able chemist Friedrich Von Stromeyer, to whom he dedicated his Elements of Chemistry; a work which has had, as it well deserves, a very wide circulation among students.

In William Farish, B. D., Jacksonian Professor of Natural and Experimental Philosophy in the University of Cambridge, the Society has lost an honorary member, elected as such soon after its original foundation, namely in November 1808, and one of a number of our countrymen who were at that period placed upon the honorary list. Professor Farish never employed himself peculiarly in geological pursuits, as we now understand the term; but it is to be recollected, that within a few years of the date of his election, which I have mentioned, the investigation of the earth's structure made a rapid progress, and, in consequence, assumed a more fixed and technical form. Professor Farish's scientific studies were mainly directed to the arts, manufactures, and machinery of the empire; on these subjects he delivered courses of lectures full of interest and instruction; and he was thus led to describe our mines, and the mode of working them.

But no reference to particular portions of Professor Farish's labours can convey a just notion of the impulse which he gave to the progress of scientific knowledge within his own sphere of influence, by the habit of seizing, with an active and vivid apprehension, upon prominent parts of modern science, and conveying them, in a manner singularly clear and simple, to his audience. For a long course of years, his lectures were more efficacious than any other circumstance in stimulating the minds of men in his university to philosophical thought on physical subjects; and to this day these lectures are never mentioned, by those who attended them at that period, without admiration and pleasure. His merit was well recognised by the university in which he spent his life. He received the highest mathematical honours of that body on taking his degree of B. A. in 1778, was elected Professor of Chemistry in 1794, and Jacksonian Professor in 1813; and at the institution of the Cambridge Philosophical Society in November 1819, he was its first president.

I cannot refrain from adding, that although I have here to speak of him principally as a man of science, such pursuits were in his case little more than episodes, in a life the main action of which was directed to the ends of religion and benevolence. In his duties as a minister of Christianity, he was most zealous and indefatigable; and every attempt to relieve the misery, the ignorance, the unjust restraints of any portion of mankind, found in him a strenuous advocate and ready agent. His childlike simplicity, genuine kindness of heart, and untiring religious earnestness, were such as well suited his kindred with Bernard Gilpin, " the Apostle of the North," from whom, through his mother, he derived his descent. He was born at Carlisle in 1759, and died at the age of seventy-eight.

Henry Thomas Colebrooke, Member of the Supreme Council of Calcutta, was one of those extraordinary men whom our Indian empire has produced;

and who shew the animating effects of the great scene in which they are there placed, by the variety of subjects to which they extend their attention, and by the vigour with which they combine speculative and practical employment. Mr Colebrooke went to India as a writer in 1782, and about 1792 began to attend peculiarly to Sanscrit literature. A little later we find him beginning to enrich the Asiatic Researches with a series of memoirs on the religion, the literature, and, above all, the science of the Hindoos. In this department his labours on the Zodiac of the Indians,[*] and on their notions of the Precession of the Equinoxes and the motions of the Planets,[†] are highly deserving of notice; as were at a later period the account of the Indian Algebra, given in his translations of the Lilawati and Vijaganita. But Mr Colebrooke was also ready to contribute a share in sciences with which we are more nearly concerned. He took a lively interest in the correction of errors respecting the physical geography of India, and was one of the first to declare (in 1815) his opinion that the Himalaya mountains were higher than the Andes, an opinion soon afterwards fully confirmed. He also was one of the first to enter upon a subject, to which we may now look with the greatest hope. The first part of vol. i. of our New Series of Transactions (published in 1822) contains two papers by him, one upon the geology of the valley of the Sutledge, which had been explored by Lieut. Gerard; the other upon the north-east of Bengal, where Mr D. Scott had noticed various rocks, and, among the rest, a deposit which contained fossils, resembling, as he conceived, those of the London clay. I shall have occasion, in the course of this address, to refer to a recent repetition of this observation of an identity between the fossils of the east of India and those of the London and Paris basin. I may observe that these, and other contributions to Indian geology by other writers, contained in the volume of which I spoke, and a preceding one, induced the Secretaries of that time to insert a map, on which the localities of these observations were indicated; and to express in the volume a hope that these were merely an earnest of the information which might be expected from the activity of British subjects in that quarter.

Among our foreign members deceased within the year, I regret much to have to mention one, to whom is due in no small degree a revolution in the mode of treating the subject of geology, which has taken place in our own times, and the formation of a new branch of geology. This revolution consists in the endeavour, now so familiar to us, to identify geological with recent changes, instead of classifying the great past changes in the surface of the earth which its structure discloses to us, as separate from the newer and slighter modifications of which history and tradition give us evidence; and the study of the discernible causes of change to which we are thus led, I shall have occasion to speak of under the name of Geological Dynamics. You are well aware that Mr Lyell is the person who has, with a bold and vigorous hand, moulded the whole scheme of geology upon this idea; but the power which he had of doing this was derived in no small degree from Von Hoff's admirable survey of the evidence of those changes which can be proved by tradition. The extent and universality of the facts thus brought into notice, might well forcibly strike a philosopher already seeking to apply such a principle to geology; and Mr Lyell has always been forward to acknowledge his obligations to M. Von Hoff. Indeed the idea of such an identification of geological with historical changes was by no means new; it had been both expressed and acted on by Deluc; and must have been present to the minds of those persons who framed the question which gave rise to Von Hoff's book. This question was proposed in 1818 by the Royal Academy of Science of Göttingen. "*Considering*," they said, "*that we have, in the crust of the earth, evidence of great revolutions, which have happened at different times, in different portions, and of which the period and duration are unknown, we are led to ask, whether certain more partial alterations may not lie within the domain of tradition, and give us the means of knowing at what period they took place, and what time the formation of certain portions of the earth's crust required; whereby some light may be thrown on those changes which lie beyond the limit of history.*"

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[*] Asiat. Res., vol. ix.

[†] Ibid. vol. xii.

M. Von Hoff's work,—"The history of those natural changes in the earth's surface which are proved by tradition"—appeared (the first part) in 1822, and had the Academy's prize assigned to it. This part of the work contained an account of the changes due to the agency of water; and, by the wide range of reading and study which it included, and the philosophic manner in which its copious materials were arranged, well justified the distinction which it received. The view presented in it of the great changes which have gone on from the beginning of historical times,—the yielding or advancing of coasts, the disappearing of islands, the union of seas,—appear to give a new face to the globe. But the portion of the judgment of the Academy which the author most valued, was that in which they said that he had used the sources of his information *conscientiously*. In 1824 appeared the second part, containing the history of volcanos and earthquakes; and, although the previous labours of Humboldt and Von Buch had done much to connect and generalise facts of this kind, Von Hoff's labours were an important step: "At least," he himself says, "he was not aware that any one before him had endeavoured to combine so large a mass of facts with the general ideas of the natural philosopher, so as to form a whole." Among other large views, we may see much which, as to kind of change supposed, agrees with the opinions of Mr Darwin, of which I shall have to speak; for instance, Von Hoff conceives that the island of Otaheite is undergoing a gradual elevation out of the sea.* Finally, the third volume of this work appeared after an interval of ten years, in 1834; in which he considers other causes of change; as rising and sinking of the land; alterations of rivers and seas; the operations of snow and ice; and also the geological results to which the whole survey had led him. In this volume he expresses his pleasure at the appearance of Mr Lyell's work, which had taken place in the intervening period, and by which he had found much new light thrown upon his own speculations.

In the interval of time between the publication of the second and third volumes, M. Von Hoff published "Geological Observations on Carlsbad" (1825), and "Measures of Heights in and near Thuringia" (1833). In this last work, he not only gave a great number of his own barometrical measurements, but discussed all extant measures of the heights of points in Thuringia, to the amount of above 1100. He also employed himself in meteorological observations.

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* Part II. Pref. p. xlv.

Karl Ernest Adolph Von Hoff, Knight of the order of the White Falcon, and invested with several offices of honour and dignity at the Ducal Court of Gotha, died at Gotha the 24th of May last. He was 66 years of age, having been born in the same city Nov. 1. 1771.

Besides the history I have mentioned, which must always continue to be a classical work on the subject of which it treats, he was at the time of his death employed in completing a continuation of his Notices of Earthquakes and Volcanic Eruptions; and also a new work, which was considered to be an important one, and was to be entitled " Germany, according to its Natural Conditions and Political Relations."

In attempting a rapid survey of the contributions to geological knowledge which have come under our notice during the past year, I may perhaps be allowed to advert to a distinction of the subject into Descriptive Geology and Geological Dynamics; the former science having for its object the description of the strata and other features of the earth's surface as they now exist; and the latter science being employed in examining and reducing to law the causes which may have produced such phenomena. We appear to be directed to such a separation of our subject by the present condition of our geological studies, in which we and our predecessors have accumulated a vast store of facts of observation, and have laboured with intense curiosity, but hitherto with very imperfect success, to extract from these facts a clear and connected knowledge of the history of the earth's changes. Nearly the same was the condition of astronomy at the time of Kepler, when the accumulated observations of twenty centuries resisted all the attempts of that ingenious man and his contemporaries to construct a science of physical astronomy. But though checked by such failures, they were not far from success; and when for the next succeeding century philosophers had employed themselves in creating a distinct science of Dynamics, the science of physical astronomy, full and complete, made its appearance, as if it were a matter of course; and thus shewed the wisdom of separately cultivating the study of causes, and the classification of facts.

DESCRIPTIVE GEOLOGY.

If we begin with geological facts, our attention should first be drawn to that district on the earth's surface within which facts have been subjected to a satisfactory comparison and classification, and which may be considered, in a general way, as including England, France, Italy, Germany, and Scandinavia. The language which the rocks of these various countries speak has been, in a great measure, reduced to the same geological alphabet. The questions of the determination of any member in one country, or the identification of similar members in two countries, are, for the most part, problems admitting of a definite and exact solution. In countries out of this district, on the other hand, we have not only to explore but to classify. We have to divine their geological alphabet;—to decipher as well as to read. We have not only to discover of what British rocks the observed ones are the equivalents, but we have to ascertain whether there be an equivalence; and, where this relation vanishes, we have to discover what new resemblances and differences of members are most worthy our notice. The great difference in the nature of the geologist's task in these two cases seems to me to make it desirable to employ the familiar division of *Home* and *Foreign* Geology in a wider sense than has hitherto been common, including in the former all that region of Europe which has had its order of strata well identified with our own; this distinction, then, I shall employ.

1. *Home* (*North European*) *Geology.*—If we attempt, in this part of our subject, to follow an order of strata, we must begin with the oldest stratified rocks, though they are undoubtedly the most obscure; for the same reason which compels the historian of states to begin with the dim twilight of their savage or heroic times; namely, because at the other extremity of the series there is no boundary; since the events of past ages and their records form an unbroken series, leading us to the unfinished occurrences and works of to-day. Going, then, as far back as the historian of the earth can discern any light, and, for reasons which may hereafter be spoken of, shaping our course by the stratified rocks alone, we should first have to ask what addition has been made during the past year to our acquaintance with those formations which have generally been called *transition.* And here, gentlemen, many of you well know, that if I had had to address you at a period a little later, I might have hoped to be able to point out, among the labours

of our members, some which may be considered as events of primary importance in this part of our knowledge;—steps which may be described as a new foundation rather than a mere extension of this portion of European geology;—a separation and arrangement of Transition rocks, which is likely to become the type and classical model of that part of the geological series, as Smith's arrangement of the oolites became the type of that portion of the strata. I speak of Professor Sedgwick's views on the Cambrian rocks, which occupy the north-west of Wales, and Mr Murchison's on the Silurian formations which cover the remainder of the principality and the adjacent parts of England. Mr Murchison's work, which cannot but be one of first-rate value and interest, will, I trust, be in our hands in a few weeks; and I should grieve to think that Professor Sedgwick will be not only so unjust to his own reputation, but so regardless of the convenience and expectations of geologists, as to withhold from the world much longer the views which his sagacious and philosophical mind has extracted from the accumulated labour of so many toilsome years, on a subject abandoned to him mainly from its difficulty and complexity.

Turning, then, to the researches which have been laid before us upon the earlier stratified rocks, I am first led to notice the important memoir of the two gentlemen I have just mentioned, upon the structure of North Devonshire. According to the views of these gentlemen, founded upon an extended examination of the county, this portion of England forms a great trough, having an east and west position, in which a series of culmiferous beds rest at their northern and southern extremities upon older rocks. The plants found in the culmiferous beds are said to be all identical with species which are abundant in the coal-fields of the central counties of England, and of the South Welsh coal basin: and it was at first conceived that these plants differed essentially from the scanty and imperfect remains of vegetables which are found in the older rocks. More recently, however, the same fossil plants which occur in the culm measures are said to have been detected in the subjacent strata. Before this fact was known, the identity of the fossils and the resemblance of mineralogical character seemed irresistibly to prove the culm-bearing beds of Devon to be the same formation with the culm or coal-bearing measures of Pembrokeshire on the opposite side of the Bristol Channel. How far this apparent anomaly admits of explanation, and in what manner it is to be allowed to modify the conclusion previously drawn, we may perhaps most properly consider as questions hereafter to be decided. The rocks which support the culmiferous formation on the north, are conceived by Messrs Sedgwick and Murchison to be a series, of which the last ascending term is probably of the date of the lowest portion of the Silurian system. On the south the culmiferous strata rest partly upon the granite, and partly upon the oldest slate rocks of Devon and Cornwall.

The same general view of the nature of the transverse section of Devon, and of the age of the culm, has been presented, perhaps I ought to say adopted, by the authors of two other papers upon the same region which have been brought before us—Mr Austen and Mr Weaver; and also, at least so far as the section is concerned, by the Rev. D. Williams, in a communication made to the British Association in September last. Nor am I aware that it has been dissented from by any one who has examined the county in question since this view was made generally known. Resting on the concurrence of so many able observers, I should conceive, therefore, that we may look upon this view as *established*, so far as the time which has elapsed allows us to use the term. No truths should be termed incontestable till a considerable period has been left for the antagonists to show themselves and to try their force.

Although this view has thus so good a claim to acceptance, you are aware, Gentlemen, that it is entirely different, both as to the form of the section and the age of the members, from that which was entertained up to the time when these gentlemen turned their attention to the subject. Their opinion respecting Devonshire being adopted, along with the views of the same eminent geologists respecting Cumberland and North and South Wales, one-third of our geological map of England will require to be touched with a fresh pencil.

Nor is this wonderful. It is rather a matter of extraordinary surprise, that when the rest of the geological map of

England is again drawn, there is scarcely any but microscopic alterations which require to be made. No higher evidence can be conceived of the vast knowledge and great sagacity of its author.

Such modifications we must ever expect to have to make of a first approximation; and I should think it a misfortune to our researches if we should attempt to elude this necessity by giving up the key of all our geological knowledge of our country,—the doctrine that there is a fixed order of strata, characterized mainly by their organic fossils. If we have not advanced so far as to prove this, what have we proved? If our terms do not imply this, what is their meaning? Is it not true, in our science as in all others, that a technical phraseology is real wealth, because it puts in our hands a vast treasure of foregone generalizations? And if we evade the difficulties which may occur in the application of this phraseology to new cases, by declaring that our terms are of little importance, is not this to deprive our language of all meaning and all worth? Do we not thus refuse to recognise as valuable the tokens which we ourselves circulate, and plainly declare ourselves bankrupts in knowledge? When certain strata of Devon have thus been identified with the coal-measures of other regions, can we still term them grauwacke? Either this term implies members having a definite place in our series of strata, or it does not. If it do, it is certain that these strata have not that place. If it do not, it conveys no geological knowledge at all. But if it be used to imply a rejection of such series, it involves a denial of all geological knowledge hitherto asserted concerning the older rocks of this county.

The transition downwards from the culmiferous beds of Devon to the older strata on which they rest, is, according to almost all who have studied the subject, wrapt in great obscurity. In this obscurity, if it be true that the fossil plants of the culm measures are found also in the subjacent rocks, there is nothing which need make us mistrust the clear and positive part of our knowledge. And even if this be so, it will not be the less necessary to separate the culmiferous from the subjacent Silurian and Cambrian systems, by a different name in our lists, and by a different colour in our geological maps, if they are to represent the present state of our information.

The interest of this question has induced me to dwell upon it longer than I had intended, and I must on that account be very brief in my notice of many other communications. I may observe that the very nature of several of these indicates very remarkably the European character which our geology has assumed, since they have for their object the identification of some members of the recognised series of England, and of France, or Germany. Thus Mr Murchison and Mr Strickland have attempted to shew, by the evidence of organic fossils, now for the first time adduced on this point, that the red saliferous marls of Gloucester, Worcester, and Warwickshires, with an included bed of sandstone, represent the keuper or *marnes irisées* of Germany; and that the underlying sandstone of Ombersly, Bromsgrove, and Warwick is part of the bunter sandstein or grès bigarré of foreign geologists. They are thus led to conclude that though the muschelkalk, which intervenes between these formations in Germany, is absent in the new red system of England, and of a large part of France, its other members may be identified over the whole of the north of Europe.

Proceeding from the new red to the oolite system, we have a memoir from Mr Pratt containing an examination of the geological character of the coast of Normandy, which necessarily implies a comparison of this series of rocks with those of England. The identification is found to be complete, as had already been believed; but Mr Pratt has made some alteration in the received doctrines on this subject; for instance, the Caen stone, which is usually considered to represent the great oolite, he finds to resemble in its fossils the inferior oolite.

Ascending still, we have to notice Mr Clarke's elaborate geological survey of Suffolk, which, of course, refers entirely to the chalk and overlying beds. With regard to the crag of this district, I may remark that M. Desnoyers, in a communication made to the Geological Society of France, has endeavoured to identify this formation with the *Faluns* of the Touraine. M. Deshayes had referred the latter to the *Miocene*, and the crag to the *Pliocene* formations of Mr Lyell. The point is one of great interest, since it involves the question of the value and right mode of application of the test of the relative number of recent species, on which Mr Lyell's classification, or at least his nomenclature, is founded. I conceive that in a matter of arrangement any arbitrary numerical character must lead to violations of Nature's classifications; and can only be considered as an artificial method, to be used provisionally till some more genuine principle of order be discovered.

Mr Clarke, in his survey, has noted as one division of the diluvium of his district, a clay of a yellowish or bluish hue, containing rolled pieces of chalk. This deposit is of great extent and thickness in East Anglia and the neighbouring parts; and is worth notice, since this deposit is one main cause of the geological confusion and obscurity in which that region is involved. In the neighbourhood of Cambridge this diluvial deposit, is called the *brown clay;* and I can state, from my own experience, that the recognition of it as a separate bed at once rendered the stratification clear, where it had long been unintelligible.

Before quitting our stratified rocks, I may notice the communications respecting some of their fossils which we have received, particularly that of Mr Williamson on the fossil fishes of the Lancashire coal-field, and the establishment of the new genus Tropæum, separated from the Hamites of the green sand by Mr Sowerby.

In attempting to pursue a stratigraphical order, we are compelled to reserve for a separate head the notice of unstratified rocks, since their age and history are only known by the mode in which they interrupt and disturb the rest of the series. We have not had many communications respecting European rocks of this character; but we cannot but be struck by the subversion of ancient ideas which results from the investigations of Messrs Murchison and Sedgwick. They have shewn that the granite of Dartmoor, and consequently that of Cornwall, formerly considered as one of the earliest monuments of the primeval ages of the earth's history, is posterior to the deposit of the culm measures.

Advancing to newer phenomena, we find the evidences of change still unexhausted. We cannot but reflect how familiar those views of the elevation and depression of portions of the earth's surface are become, which were at first considered so strange and startling. This is remarkably shown by the number of communications concerning raised beaches which we have recently received. When we visit places where these occur, and look at the winding shore, where the sea line is faithfully followed or distinctly imitated by terraces, sands and pebbles a little above it, we wonder that we should so long have been blind to this kind of evidence. Such raised beaches have been described during the past year, by Mr Prestwich, as occurring in the Murray Firth: by Mr Austin, in the valley of the Axe, the Exe, and the Otter. Dr Forchammer has given us the evidence of recent elevation in the island of Bornholm; Mr Trevelyan has given us similar evidence for the coast of Jutland, and the islands of Guernsey and Jersey.

Mr Morris's paper, describing a series of dislocations in the chalk cliffs to the south of Ramsgate, marked by shifts in a bed of tabular flint, may perhaps be considered as also affording evidence of violent elevation. But since a small derangement of the conditions of support of any stratum might occasion dislocations of the scale of those here described, it would probably be hazardous to consider them as otherwise than local accidents.

Among descriptions of the most recent geological phenomena, I must notice Mr Clarke's paper on certain peat-marshes and submarine forests, which occur near Poole in Devonshire; and in his investigation of the causes which have produced the results now visible, we may see by how easy a gradation descriptive geology passes into the other portion of the subject, the study of the processes by which change is produced.

Finally, in concluding this survey of our descriptive home geology, I notice with great pleasure, Mr Burr's communication of his notes on the geology of the line of the proposed Birmingham and Gloucester Railway. In a country like this, in which the order and boundaries of the strata are, for the most part, well ascertained, an additional accuracy of measurement, of great value to us, may be supplied by the operations of civil engineers employed on canals, roads, and the like works. With this persuasion, and acting with the advice of the Council, I wrote letters to a great number of engineers, begging them to communicate to us the levels and sections which they might obtain in the course of their professional employments; and I am happy to see so excellent an example as Mr Burr's paper supplies, of the advantage which may be derived from materials of this class.

2. *Foreign (South European and Trans-European) Geology.*—In proceeding beyond the Alps, and still more as we advance beyond the shores of Europe, we can no longer, so far at least as geologists have hitherto discovered, trace that remarkable correspondence of the strata of different countries which we can study so successfully in our *home circuit.* With the mountain masses of those more distant regions we are, it would seem, hardly authorised as yet in making any more detailed distinctions than the general one of secondary and tertiary strata; the latter including the strata in which we trace an approach to the existing species of animals, and the former implying a general comparison with our chalk, oolites, and lower strata. Perhaps we may further distinguish in most countries which have been visited, a great mass of transition states; but the establishment of such divisions must be the business of geological observers.

We have had several valuable additions to this portion of our knowledge, including, as we must do, Greece and its islands in this foreign district. That the Apennine limestone is the predominant mass of the Morea, had been made known by the researches of MM. Boblaye and Virlet. Mr Strickland and Mr Hamilton have told us that the same rock forms a large mass of the island of Zante and other islands in that sea, and of the neighbourhood of Smyrna. They find also tertiary beds, as on the south side of the bay of Smyrna; on the east side of the island of Zante; and at Lixouri in Cephalonia, where the tertiary beds are remarkable for the number and beauty of their fossils, some of which have been identified with species existing in the Mediterranean. Dr Bell, who travelled from Teheran to the shores of the Caspian, has given us an account of the rocks which he observed in Mazanderan. From the statements made by him, we are led to believe, that a more continued and detailed observation of the country would give the true geological order of the deposits in this region; which might then, perhaps, serve as a connecting link between western Asia and India.

It is among the favourable omens for the geology of India, of which we now see many, that a temperate spirit of generalization has recently been applied to the examination of her soil; a spirit which contents itself with such a general reference of the foreign to the home strata as we have described, till by its own labours it has earned the right of asserting some closer correspondence. If to deny the value of our geological terms within the home district, where they mark an order which has been repeatedly verified, would be a suicidal scepticism in geologists, there would be a rashness and levity no less fatal in applying them to distant regions where no order has yet been ascertained.

Captain Grant in his account of Cutch, and Mr Malcolmson in his description of a large portion of the India peninsula, have not ventured to call the strata which they have examined by the names which describe European formations. We may trust that, hereafter, the admirable activity and resource which our countrymen display in that wonderful appendage of our empire, will enable them to communicate to us a genuine Indian arrangement of secondary strata. In the mean time, Mr Malcolmson has most laudably employed himself in determining the age of the wide-spread igneous rocks of the peninsula of India, with reference to the contiguous strata. And Dr McCleland, who was associated with Mr Griffith in the scientific deputation sent under Dr Wallich into Upper Asam, has, among other geological observations, noted a raised bed, at 1500 feet above the sea level, in which none of the species are identical with those of the Bay of Bengal on the one hand, or the secondary strata on the north of the Himalaya on the other; but in which a resemblance was at once recognised with the species of the Paris basin.

This resemblance between the extinct animal population of regions so remote from each other, is in itself remarkable enough. It is still more curious to observe, that the same coincidence of the ancient animals of France and India has recently been detected in another case; and what makes the circumstance still more remarkable is, that the animal was not only new in both countries as a fossil genus, but involved a transgression of the supposed boundaries of fossil forms. Not only had no human bones been found in genuine strata, but, as it had been generally held, no traces of those creatures which most nearly imitate the human form. This rule now no longer holds good; for during the past year the bones of monkeys have been discovered both at Sansan, in France, in the Sewalik Hills in the north of Hindostan, and more recently under the City of Calcutta.

That this is a highly interesting and important discovery, no one who attends to the signification of geological speculations can doubt. I do not know if there are any persons who lament, or any who exult, that this discovery tends to obliterate the boundary between the present condition of the earth, tenanted by man, and the former stages through which it has passed. For my own part I can see no such tendency. I have no belief that geology will ever be able to point to the commencement of the present order of things, as a problem which she can solve, if she is allowed to make the attempt. The gradation in form between man and other animals a gradation which we all recognise, and which, therefore, need not startle us because it is presented under a new aspect, is but a slight and, as appears to me, unimportant feature, in looking at the great subject of man's origin. Even if we had no divine record to guide us, it would be most unphilosophical to attempt to trace back the history of man without taking into account the most remarkable facts in his nature; the facts of civilization, art, government, writing, speech—his traditions—his internal wants—his intellectual, moral, and religious constitution. If we will look backwards, we must look at all these things as evidences of the origin and end of man's being. When we do thus comprehend in our view the whole of the case, it is impossible for us, as I have elsewhere said, to arrive at an origin homogeneous with the present state of things; and on such a subject the geologist may be well content to close his own volume, and open one which has man's moral and religious nature for its subject.

In order to complete the notice of the contributions to foreign geology, I must mention Mr Roy's account of Upper Canada;

in which country he conceives that he has detected terraces which exhibit the beaches of the lakes when the level of their surface was more elevated than they are at present. I must refer also to Mr. Bollaert's paper on alluvial accumulations containing large masses of silver ore in Peru. And, finally, I have to direct your attention to the very curious information respecting the geology of South America, which we have received from Mr Darwin. In a communication made to us, he gave a very striking view of the structure of a large portion of that continent; and, as I have already had occasion to observe, he has brought to this country the remains of various fossil animals of entirely new kinds, of exceeding interest to the zoologist as well as the geologist. I need only remind you of the gigantic mammifer which has been reconstructed in idea by Mr Owen, upon the evidence of a fossil skull, and has been named by him the *Toxodon Platensis.* This animal, although a *Rodent*, according to its dental characters, in other respects manifests an affinity to the *Pachyderms;* and also to the *Dinotherium*, and to the *cetaceous* order. Many other fossil animals have been discovered in South America; and all, from their magnitude, fitted to excite our wonder, when we compare the diminutive size of the present races of animals which inhabit that country. The animal remains found by Mr Darwin comprise, besides the Toxodon, which extraordinary animal was as large as a hippopotamus,—(2, 3, 4, 5, 6.) the Megatherium, and four or five other large Edentata;—(7.) an immense Mastodon;—(8.) the Horse;—(9.) an animal larger than a horse, and of very singular character, of which a fragment of the head has been found;—(10, 11, 12.) parts of Rodents, one of considerable size;—(13.) a Llama, or *Guanaco*, fully as large as the Camel.

But I should very ill convey my impression of the great value of the researches of Mr Darwin, by any enumeration of special points of geology or palæontology on which they have thrown light. Looking at the general mass of his results, the account of which he has been kind enough to place in my hands, I cannot help considering his voyage round the world as one of the most important events for geology which has occurred for many years. We may think ourselves fortunate that Capt. Fitz-Roy, who conducted the expedition, was led, by his enlightened zeal for science, to take out a naturalist with him. And we have further reason to rejoice that this lot fell to a gentleman like Mr Darwin, who possessed the genuine spirit and zeal, as well as knowledge of a naturalist; who had pursued the studies which fitted him for this employment, under the friendly guidance of Dr Grant at Edinburgh, and Professor Henslow and Professor Sedgwick at Cambridge; and whose powers of reason and application had been braced and disciplined by the other studies of the University of which the latter two gentlemen are such distinguished ornaments. But some of the principal of these results may be most conveniently mentioned, when we pass from mere descriptive geology, to that other division of the subject which I have termed Geological Dynamics. And this I now proceed to do.

GEOLOGICAL DYNAMICS.

This term is intended to express generally the science, so far as we can frame a science, of the causes of change by which geological phenomena have been produced. Without here speaking of any classification of such changes, I may observe that the gradual elevation and depression, through long ages, of large portions of the earth's crust, is a proximate cause by which such phenomena have been explained: and this class of events, its evidence, extent, and consequence, is brought before our view by Mr Darwin's investigations, with a clearness and force which has, I think I may say, filled all of us with admiration. I may refer especially to his views respecting the history of coral isles. Those vast tracts of the Pacific which contain, along with small portions of scattered land, innumerable long reefs and small circles of coral, had hitherto been full of problems, of which no satisfactory solution could be found. For how could we explain the strange forms of these reefs; their long and winding lines; their parallelism to the shores? and by what means did the animals, which can only work near the surface, build up a fabric which has its foundations in the deepest abysses of ocean? To these questions Mr Darwin replies, that all these circumstances, the linear or annular form, their reference to the boundary of the land, the clusters of little islands occupying so small a portion of the sea, and, above all the existence of the solid coral at the bottom of deep seas, point out to us that the bottom of the sea has descended slowly and and gradually, carrying with it both land and corals; while the animals of the latter are constantly employed in building to the surface, and thus mark the shores of submerged lands, of which the summits may or may not remain extant above the waters. I need not here further state Mr Darwin's views, or explain how corals, which when the level is permanent fringe the shore to the depth of twenty fathoms, as the land gradually sinks, become successively encircling reefs at a distance from the shore; or barrier reefs at a still greater distance and depth; or when the circuit is small, lagoon islands:—how, again, the same corals, when the land rises, are carried into elevated situations, where they remain as evidences of the elevation. We have had placed before us the map, in which Mr Darwin has, upon evidence of this kind, divided the surface of the Southern Pacific and Indian oceans into vast bands of alternate elevation and depression; and we have seen the remarkable confirmation of his views in the observation that active volcanos occur only in the areas of elevation. Guided by the principles which he learned from my distinguished predecessor in this chair, Mr Darwin has presented this subject under an aspect which cannot but have the most powerful influence on the speculations concerning the history of our globe, to which you, Gentlemen, may hereafter be led. I might say the same of the large and philosophical views which you will find illustrated in his work, on the laws of change of climate, of diffusion, duration and extinction of species, and other great problems of our science which this voyage has suggested. I know that I only express your feeling when I say, that we look with impatience to the period when this portion of the results of Captain Fitz-Roy's voyage shall be published, as the scientific world in general looks eagerly for the whole record of that important expedition.

And I cannot omit this occasion of mentioning with great gratification, the liberal assistance which the Government of this country have lent to the publication of the discoveries in natural history which Mr Darwin's voyage has produced. The new animals which he has to make known to the world will thus come before the public described by the most eminent naturalists, and represented in a manner worthy of the subject and of the nation. I am sure that I may express the gratitude of the scientific world, as well as my own, for this enlightened and judicious measure.

I may here notice Mr Darwin's opinion, so ably exposed in a paper read before us, that the change by which a variety of materials thrown on the earth's surface become vegetable mould, is produced by the digestive process of the common earth-worm.

I will here also advert to Mr Fox's paper on the process by which mineral veins have been filled up. This, he conceives, might be produced by the circulation or ascension of currents of heated water from the deeper parts of the original fissures. The discovery of the causes of the formation and filling of metallic veins, one of the earliest subjects of geological speculation, will remain propably as a problem for its later stages, when our insight into the laws of slow chemical changes is far clearer than it is at the present day.

If, from these proximate causes of change of which I have spoken, we proceed to those ulterior causes by which such events as these are produced,—to the subterraneous machinery by which islands and continents appear and vanish in the great drama of the world's physical history,—we have before us questions still more obscure, but questions which we must ask and answer in order to entitle ourselves to look with any hope towards geological theory. Of late years an opinion has taken root among us, that the dynamics of geology must invoke the aid of mathematical reasoning and calculation, as the dynamics of astronomy did, at the turning point of its splendid career. Nor can we hesitate to accept this opinion, and to look forwards to the mathematical cultivation of physical geology as one of the destined stages of our progress towards truth. But we must remember, that in order to pursue this path with advantage, we have, in every instance, two steps to make, each of which demands great sagacity, and may require much time and labour. These two steps are, to *propose* the proper problem, and to *solve* it. Last year an important example of this kind was brought under your notice by my predecessor. The supposition that there are, beneath the crust of the terrestrial globe, liquid or semiliquid masses which exert a pressure upwards, leads to the inquiry what phenomena of fissure, disruption, and dislocation, this subterraneous strain would produce. The answer to this inquiry must be given by mathematical reasoning from mechanical principles; and Mr Hopkins, who proposed, and to a considerable extent solved this problem, has put forth a set of results, with which, so far as they are definite and decisive, it will be highly important to compare the existing phenomena of disturbed geological districts. The same assumption, of an incandescent mass existing deep below the earth's surface, has led two other distinguished members of our body to another train of speculations; which, however, though highly interesting, I should be disposed to consider as only the enunciation of a problem, requiring no small amount of mathematical skill for its solution. I speak of the speculations of Professor Babbage and Sir John Herschel, concerning the subterraneous oscillations of the isothermal surfaces of great temperature. They remark that such oscillations will arise, when thick and extensive deposits take place on any parts of the surface of the earth (as for instance at the bottoms of seas), because such deposits increase the thickness of the coating over a given subterraneous point; and thus removing the cooling effect of the surface, bring a high temperature to a place where it did not exist before. The deposited strata might thus be invaded by violent heat advancing from below; and there might result both changes of position arising from extension and contraction, and a metamorphic structure in the rocks themselves. It is highly instructing to have this chain of conceivable effects pointed out to us; but we may venture to observe that, in order to render the suggestion of permanent use, it will be necessary to express, in some probable numbers, the laws of the result as affected by the conductivity of the earth's mass, the rate and thickness of the deposit, and other circumstances. For instance, we know that a deposit of one thousand feet thick would be quite insufficient to occasion a metamorphic operation in its lower strata. Would, then, a deposit of ten thousand or of twenty thousand feet call into play such a process? To answer questions like these, of which a vast number must at once occur to our minds, we have many experimental data to collect, many intricate calculations to follow out. And it would be easy to point out problems of a still more abstruse kind, in which we no less require aid from the mathematician, before we can proceed in our generalizations. May we not hope to see some fortunate man of genius unveil to us the mechanics of crystalline forces? And when that is done, can we doubt that we shall have a ray of new light thrown upon those extraordinary phenomena of slaty cleavage in mountain masses which have lately been brought under our notice? Or, recollecting the experiments of Sir James Hall (a striking step in geological dynamics), may we not hope then to learn how those crystalline forces are stimulated by heat; and thus follow the metamorphic process into its innermost recesses? These and a thousand such questions lie before us; —tangled and arduous inquiries no doubt, but connected by their common bearing upon one great subject:—"a mighty maze, but not without a plan." And through this maze we must force our way in order to advance towards any sound geological theory. The task is one of labour and difficulty; but I well know, gentlemen, that you will not shrink from it on that account. Those who aspire to the felicity of knowing the causes of things, must not only trample under foot the fears of a timid unphilosophical spirit, which the poet deems so necessary a preparation, but they must look with a steady eye upon difficulty as well as violence. They must regard the terrors of the volcano and the earthquake, the secret paths by which hot and cold and moist and dry run into their places, the wildest rush of the fluid mass, the latent powers which give solidity to the rock,—as operations of which they have to trace the laws and measure the quantities with mathematical exactness. And though there can be no doubt that the greater part of us shall be more usefully employed in endeavouring to add to the stores of descriptive geology, than in these abstruse and difficult investigations, yet we must always receive, with great pleasure, any communications containing real advances in the mathematical dynamics of geology, from those whose studies and whose powers enable them to lay an effectual grasp upon these complex and refractory problems.

I have but a single word to add in conclusion. This Society has always been an object of my admiration and respect, not only from the importance and range of its scientific objects, the wide and exact knowledge which it accumulates, the philosophical spirit which it calls into play, the boundless prospect of advance which it offers; but also for the manner in which its meetings and the intercourse of its members have ever been conducted; the manly vigour of discussion, tempered always by mutual respect and by good manners; the deep interest of all in the prosperity of the Society, to which, whenever the hour of need comes, private differences of opinion and resentments have given way. To be placed for a time at the head of a body which I look upon with such sentiments, I must ever consider as one of the greatest distinctions which can reward any one who gives his attention to science. I trust, by your assistance and kind sympathy, gentlemen, I shall be able to preserve the spirit and temper which I so much admire;—to hand that torch to my successor burning as brightly as it has hitherto done. And there is one consideration which will make me look with an especial satisfaction upon such a result. I have not myself the great honour of being one of the members of the Society who are connected with it by an early interest in its fortunes, and by long participation in its labours. I may consider myself as only belonging to its second generation. Now if there be a critical and a perilous time in the progress of a voluntary association like ours, it is when its administration passes out of the hands of its founders into those of their successors. It is like that important and trying epoch when the youth quits the paternal roof. I will say, however, gentlemen, for myself and for my fellow-officers, some of whom are in the same condition, that our best cares shall not be wanting that the Society may suffer as little as possible by this change. And among our grounds for hope and trust, the main one is this: that though the offices of the Society may be in younger hands, the parental cares of its founders are not withdrawn. We have to discharge our office with the aid and counsel of those excellent persons to whom the prosperity of the Society up to the present time has been owing. Surrounded by such men, knowing their generous and ready sympathy for the attempts and exertions of their followers and disciples, I feel a cheerful confidence in the future destinies of the Geological Society; and a persuasion that it will not only preserve but extend its influence as a bond of scientific and social union among its members.

Result of the Examination of the Sea-Water collected during the Voyage of La Bonite, by means of the apparatus of M. Biot. By M. DARONDEAU.

THE samples of sea-water collected by means of M. Biot's apparatus,* and brought to France for analysis, were five in number. Two of them were taken in the Bay of Bengal, not far from the mouths of the Ganges, and the three others, respectively, from the Pacific, the Indian, and the Southern Atlantic Oceans. They were contained in stopped flasks made of emery, and did not fill above two-thirds, which circumstance arose from the flasks at our disposal being of a larger size than the receiver of our apparatus. Five other samples, taken from the surface of the water, were also procured in the same localities; they, too, were contained in emery bottles, which were completely filled. One of the bottles, namely, that which contained the water taken at the surface in the Southern Atlantic, was broken in its conveyance from Brest to Paris.

All the specimens taken at the surface were perfectly limpid; whilst, on the contrary, those taken at a considerable depth held some whitish flaky matter in greater or less quantity in suspension.

All the experiments upon these different samples were made in the laboratory of the *Collége de France*, under the inspection and with the assistance of M. Frémy, to whose kindness I am indebted for my now having it in my power to supply the results.

The density of the water was determined by successively weighing the emery flasks first empty, then filled with distilled water, and lastly, with the sea-water, and then comparing the weight of the two equal volumes of the distilled and sea-water. These weighings were made at temperatures which varied from 45° 5′ to 50° Fahrenheit.

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* Our readers will find an account of M. Biot's apparatus in this Journal, Vol. 21, p. 43.—EDIT.

The quantity of the gas held in solution in the water, was determined by heating it to boiling in a retort of known capacity, which was quite filled with the water; the gas disengaged in this operation was collected over mercury; the proportion of carbonic acid it contained was saturated by means of potassa, and the oxygen by means of phosphorus. Finally, to ascertain the quantity of saline matter, the method which M. Gay-Lussac describes in the fourth volume of the *Annales de Physique et de Chimie*, was pursued, which consists in evaporating to dryness a known weight of sea-water in a retort whose weight is known, and which is inclined at an angle of 45°, so that there may be no escape of any matter externally. The weight of the residue, heated to a dull red, gives the quantity of saline matter, less the quantity of chloriodic acid, arising from the decomposition of the chloride of magnesia by the heat, whose quantity is ascertained by determining the quantity of magnesia contained in the residuum, and by replacing in this magnesia the oxygen by its equivalent of chlorine. By operating in this way, the results indicated in the following table were procured:—

Times and Places at which the Water was taken.	Latitude.	Longitude.	Depths at which procured.	Density at 46° 4′ and 56° Fah.	Residue of Saline Matter in every 100 parts water.	Quantity of Gas in every 100 parts; water at freezing point, and 760 m.m pressure.	Composition of 1 parts of Gas.		
							Oxyg.	Nitro.	Car.
30th Aug. 1836, Pacific.	11° 8′ N.	108° 50′ W.	Surface,	1.02594	3.429	2.09	6.16	83.33	10.5
			70 fath.	1.02702	3.528	2.23	10.09	71.05	18.0
19th March 1837, Bay of Bengal.	11 43 N.	87 18 E.	Surface,	1.02545	3.218	1.98	5.53	80.50	13.9
			200 fath.	1.02663	3.491	3.04	3.29	38.56	58.1
10th May 1837, Bay of Bengal.	18 0 N.	85 32 E.	Surface.	1.02611	3.378	1.91	6.34	[illegible]	13.3
			300 fath.	1.02586	3.484	2.43	5.72	64.15	30.1
31st July 1837, Indian Ocean.	24 5 S.	52 0 E.	Surface,	1.02577	3.669	1.85	9.84	77.70	12.4
			450 fath.	1.02739	3.518	2.75	9.85	55.23	34.9
24th Aug. 1837, South. Atlantic.	30 40 S.	11 47 E.	......	......	...	...	...	...	...
			400 fath.	1.02708	3.575	2.04	4.17	67.01	28.8

The figures in the above table shew that, generally, the density of water taken at the surface is less than that of water procured from a certain depth. In one case only, viz. that in which water was taken in the bay of Bengal from a depth of 300 fathoms, was its density less than when taken at the surface; the difference amounted to $\frac{3}{10000}$.

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* There is much uncertainty in this experiment regarding the quantity of the carbonic acid gas, because it was not immediately saturated.

If we consider the proportion of the residuum left after the desiccation, it will be observed, as in the preceding case, that sea-water, generally, is considerably salter at a certain depth than at the surface. There is one example of the contrary in the table. Notwithstanding, these results do not appear to be inadmissible; for there is a great difference between the temperature of the water at the surface, and of that at the depth of from 800 to 400 fathoms; the equilibrium must always be preserved.

As to the quantity of air held in solution in the water, the table shews that the water taken at the surface in all cases contains a smaller proportion of air than that which is taken from a certain depth, and that the difference may amount to as much as to one-hundredth part of the volume of water.

Finally, the column which indicates the composition of the gas contained in each of the samples of water, shews that the gas contained in the water taken from a great depth, contains much more carbonic acid than is contained in water taken from the surface. Does this carbonic acid exist already formed in the water, or rather, does it proceed from the decomposition of the flaky matter which is found in all the flasks which have been filled at a great depth? This can be determined only by analysis conducted on the spot. And whatever may be the result, M. Biot's apparatus will at all events be the means of confirming one or other of these equally remarkable facts; 1*st*, That sea-water at a certain depth, holds in solution a much greater quantity of carbonic acid than does water taken at the surface; or, 2*d*, That at a certain depth the water contains many transparent animalculæ; or at least some organic matter which does not exist at the surface, and which decomposes with time, and takes from the air, which is held in solution in the water, its oxygen, so forming carbonic acid.

According to this latter hypothesis, the proportion of oxygen contained in the air taken from a great depth, should be more considerable than that of the air taken from the surface-water; for, in the former case, the free oxygen and the oxygen of the carbonic acid form with the contained nitrogen a much more oxygenated air than the atmospherical; whilst in the latter case (that of water taken at the surface), the free oxygen and the oxygen of the carbonic acid, form with its nitrogen an air which differs exceedingly little from that of the atmosphere.

Some Experiments which were made on board La Bonite.

In an experiment made on the 12th September 1836, in the Pacific, at 16° 53′ N. lat. and 118° 13′ W. long. with water taken from a depth of 380 fathoms, it was found to contain 1.62 parts of gas for every 100 parts of water; the gas was not analyzed. In this experiment the vessel contained 90.66 cubic centimetres of air, whose volume reduced to the freezing point of temperature, and a pressure of 2280 feet, gives, having regard to the capacity of the apparatus, 6.48 parts of air for the 100 parts of water taken at the depth of 380 fathoms.

On the 21st November 1836, in the Strait betwixt the Marianne and Phillipine Islands, in 18° 22′ N. lat. and 132° 13′ E. long. the apparatus was sunk to the depth of 300 fathoms; the water from this depth contained 2.20th parts of air in every 100 parts of water; whilst the water taken at the surface in the same Strait contained 2.27ths; the bladder contained only a very small quantity of air.

Finally, on the 29th November, in the China Sea, in sight of the Island of Luçon, in 18° 0′ N. lat. and 117° 30′ E. long. the instrument having been sunk to a depth of 300 fathoms, the bladder was found to contain 55 cubic centimetres of air, which, at 0° and 760 of the barometer, gives 3.89 parts for 100 parts of water taken from this depth.—*Comptes Rendus*, 30. *Avril* 1838.

Observations on the Electric Origin of Metalliferous Veins. By M. Becquerel.[*]

Mr Fox remarks, that, if we consider the electrical relations of the different metallic ores in a geological point of view, we observe, that almost all those which are generally associated in the same veins agree in the particular, that their reciprocal voltaic action is, for the most part, very small. Hence he infers, that, if it were otherwise, the appearances of decomposition in the same localities would be much more marked and general than they are found to be. He remarks also, that when copper pyrites and vitreous copper-ore form a voltaic combination with pure or spring water, there are considerable electro-magnetic effects. We would merely observe, that Mr Fox appears not to be aware, that the electro-chemical effects produced in the contact of two solid bodies and a liquid, depend solely on the chemical relations of their constituent parts, and must frequently vary.

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[*] Traitè Expèrimental de l'Electricitè et du Magnetisme, tome v.p. 167, 174. The notes marked W. J. H. were furnished by Mr Henwood, to whom, also, we are indebted for this translation.

Let us now approach the researches of the same gentleman on the electro-magnetic properties of the metalliferous veins of Cornwall, in which he has been engaged for six years past.

The apparatus which he has used in examining these properties is composed of small plates of copper, fixed on the portions of the veins submitted to experiment with iron nails,* or strongly pressed by means of wooden supports, and put in connexion with the extremities of a multiplier, of which the wire is short, and the directive power of the needle not neutralized.

Mr Fox says, that with this apparatus he perceived the following effects:—The intensity of the current varies, according to the localities. Sometimes the deviation of the magnetized needle is slight, sometimes it is considerable. In general, it is greater when the vein contains a larger quantity of copper, and perhaps even from the depth of the stations. He adds, that there is no, or scarcely any, action perceptible where there is little or no metallic substance. When there is a distance of but a few fathoms between the plates in a horizontal direction, and when there is between them a great quantity of copper not interrupted by non-conducting substances, there is no action; but if there be, by chance, quartz or clay in the vein, the action is generally very decided.

When the two plates are placed at various depths in the same vein, or in different veins, the electrical action is in general very marked. The currents are sometimes in one direction, sometimes in another. In comparing parallel veins, he thinks he has observed that positive electricity takes a direction from north to south, although he has seen the contrary in some cases. In veins which dip towards the north, the east is generally positive, and the west negative. He has found, in comparing the relative states of veins at different depths, that the lower stations appear negative in relation to the higher. He has, however, found some exceptions, particularly when a *cross-vein* of quartz or clay intervenes between the plates. There is, therefore, no regular order in the direction of the currents.

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* Nails were very seldom used, and those that were were always of *copper*. W. J. H.

If, indeed, there were a progressive increase of negative electricity as we descend in the mines, this phenomenon would agree with the progressive elevation of temperature. The electrical effects are not influenced, according to his account, by the presence of workmen and their lights, or by the explosion of gunpowder, &c.

All the substances that form part of metalliferous veins are far from possessing the conducting powers necessary to allow the passage of currents transmitted by the metallic portions. He classes among the conductors copper nickel, copper pyrites, vitreous copper-ore, iron pyrites, arsenical pyrites, galena, arsenical cobalt, the crystallized peroxide of manganese, and tetrahedral copper-glance (Fahlerz). Among the non-conductors he places the sulphurets of silver, of mercury, of antimony, of bismuth, of arsenic (realgar), of manganese, and of zinc, the combinations of the metals with oxygen and the acids.

Mr Fox assures us, that he has discovered that the beds of clay-slate (killas) in Cornwall appear to possess the property of conducting free electricity in a slight degree, but only in the direction of their cleavage. This effect can be attributable only to the water interposed.

With regard to the electric properties of metallic veins, he remarks, that substances which conduct electricity have generally, at least in this country (Cornwall), non-conducting bodies intervening in the veins, between them and the surface. He mentions the tin veins, which are generally intersected by those of copper. When their horizontal bearings do not coincide, the conducting veins traverse the non-conductors.

Mr Henwood, who has been engaged since Mr Fox in experiments on the electric currents in the Cornish mines, has stated, that the veins on which the mines are worked traverse both the granite and the micaceous rocks (slates); that they are principally composed of quartz and other earthy minerals, mixed in many places with copper pyrites, iron pyrites, vitreous copper-ore, oxide of tin, blende, and galena, and occasionally with native copper, protoxide and the carbonates of the same metal, and of some of the salts of lead, in small quantities.

At the greatest depths, the temperature of the slate-rocks is two or three degrees higher than that of the granite at the same level.

In many of the deepest mines, the water contains salts in various proportions. Among others, the chlorides of calcium, of sodium, and of magnesium, &c.

Mr Henwood has adopted the mode of experiment already described. The metallic plates were placed at distances varying from a few feet to many hundred feet, at the same, and at different levels.

The results have been the same whatever the directions of the veins might be. In those which only produced tin, and in many where they were in contact with copper, no traces of a current were perceived, except in some cases where the intermediate space was filled with rich copper-ore.* The presence of electricity was more evident when the vein contained copper-pyrites, vitreous and black copper-ore, galena, or blende. It was not detected when the vein was entirely without metal. Some veins which contained copper-pyrites, grey copper-ore, and galena, and others with carbonate and phosphate of lead, and grey copper-ore, gave no evidence of the existence of currents.

It appears that Messrs Fox and Henwood have not remarked the relations which subsist between the directions of the veins and those of the currents.†

In the experiments where they have connected metalliferous portions at different depths, the currents have in thirteen instances been upward, and in thirty-five cases downward.

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* "In most cases where there was a continuous mass of copper-ore between the points examined, no electricity was detected. In some instances, however, where all the intervening space consisted of rich copper-ore, there was most energetic action."—*Edin. New Phil. Journal*, xxii. p. 274.

† There must be some misprint or misapprehension here, as the directions of the veins have been mentioned in paragraph preceding.—W. J. H.

In thirty-six experiments the current has been towards the granite, and in twenty-one others it has taken an opposite direction.

We are not aware whether Messrs Fox and Henwood have, in their experiments, sufficiently guarded against all the causes of error which present themselves when we seek for the existence of electro-chemical currents by means of the multiplier. The results they have announced are of such importance as regards the electro-chemical re-actions which operate in veins, that we must discuss them.

To enable the reader to judge and appreciate the accuracy of Mr Fox's process, we will remind him, that when two plates of platina, in communication with the extremities of the wire of a multiplier, are immersed in distilled water, a current is immediately produced by the difference of the actions exercised by the liquid on the foreign bodies adhering to the surfaces of the plates. This effect almost invariably takes place when the precaution is not taken to remove the foreign substances which adhere to the surfaces of the platina when they are taken out of the water. If, instead of platina, we employ copper, the current is still more decided. As the surfaces are not the same, the water affects them differently.

Now Mr Fox, in his experiments, has used plates of copper and he has attached them to the metalliferous veins with iron* nails, or strongly pressed them on by means of wooden stays. These two plates are connected with a multiplier by means of a copper wire.

Might it not happen, that the water which adheres more or less to the sides of the mine galleries, and which does not every where contain the same salts, might have the same effect on the copper as in the experiment just described? It would be very desirable for Mr Fox to make his experiments on perfectly dry conductors of electricity. The objection which we have just made would thus be obviated.†

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* Note on page 168.

† There was no moisture except in the air, where a current was detected in *Levant Mine*. W. J. H.

It is true, Mr Fox, who has operated with plates of copper and of zinc alternately, has observed in both cases currents taking the same direction. Such a result is so far favourable to his opinion, but it is not sufficient to demonstrate completely the fact.

In order to shew in what manner electric currents have acted, and are acting, on metalliferous deposits, some persons have recently supposed that the veins have been filled by the action of electric currents; but it is only necessary to be acquainted with the manner in which they are filled, to reject such a theory.

We know that veins are fissures which occur in many of the rocks composing the crust of our globe, and that they are filled with metallic and stony substances. Different opinions are held as to the manner in which this was effected. Some say by igneous, others by aqueous action. Werner was of the latter opinion. According to this celebrated geologist, the mass of which mountains are composed was moist and yielding; afterwards, in settling and drying, fissures were formed, which were filled from above by substances held in solution; but, as there are veins which appear to have been filled from beneath, we must therefore admit that this was effected by sublimation. Hutton, who is the great advocate of the igneous theory, supposed the internal heat of the earth so great, as to melt and volatilize the metals and earths. These, by their expansive force, have produced rents in the crust of the earth, and solidifying during their escape, have thus given birth to the crystalline rocks. It is thus he accounts for the production of the great trap-dykes which traverse formations of all ages.

Taking this view, veins have been opened by elevatory action, and filled by sublimation from beneath, and from above by substances which have been decomposed and transported by various causes, and have rested undisturbed in the veins.

All the facts hitherto observed induce us to think, that we cannot admit one of these hypotheses to the exclusion of the other, as each of the supposed causes may have concurred, according to circumstances, in filling veins of various descriptions. Geologists consider it certain, that veins containing the debris of the superior (or newer) formations of the country they traverse, and of organized bodies, have been filled from above;

but the case is different with respect to veins which are connected by mineral transitions with the contiguous rocks. In the latter we are obliged to suppose, that the formation of the rock, that of the vein and its filling, are almost contemporaneous. On the other hand, when we see the crystalline masses of different substances in the middle of, and on all sides entirely surrounded by rocks equally crystalline, so that we cannot believe they have been introduced into the cavities they fill, either from above or beneath, we are compelled to consider the vein as an open fissure in a crystalline rock, that has been again penetrated by the substance in solution, which has thus been introduced into the cavity and precipitated there, the various parts of the rock being more or less dense, or the solution being more or less saturated in different situations. There is another mode of filling, which has produced the veins which contain metallic sulphurets, in crystalline groups, projecting in all directions in the vein, and bodies which decompose in aqueous solutions, such as the metallic sulphurets and arseniurets, which will not sustain an elevated temperature without decomposition, unless under the action of a considerable compressing force.

It therefore appears almost certain, that veins have not all been produced by one general cause alone, and that many influences have sometimes concurred in their formation.

It follows, from the brief sketch which we have just presented of the state of our knowledge of the constitution of veins, that it is impossible to admit, that the fissures which have at different periods opened in the rocks have been filled by substances transported thither by the action of terrestrial electric currents, as they exert no chemical action except where solid conducting bodies and liquids, capable of reacting one on the other, exist. Now the rocks are not conductors of electricity, and the solid metallic ores were not in existence when these fissures were formed. We must therefore admit, that other causes than electric currents have filled these rents. The filling up once effected, either entirely or in part, and the water entering from the surrounding rocks, electric forces would then intervene to effect decompositions, and give birth to new combinations.

Geographical and Geological Observations on some parts of European Turkey, namely, Mæsia, Bulgaria, Romelia, Albania, and Bosnia. By Dr A. Boué. Communicated by the Author in a Letter to the Editor.*

In the general view of the Orography of European Turkey, communicated in the preceding volumes of this Journal, I could only enter into a few details regarding Servia, Macedonia, and some parts of Romelia and Mæsia Superior. Last year, however, I visited the other provinces of the empire, and directed my attention especially to the eastern and western ranges of mountains, the Hæmus, and the Albanian-Bosnian chains; whilst I endeavoured, at the same time, to complete my knowledge of the central chains, from the Dardanelles to the Adriatic. I now possess nearly 350 barometrical measurements, with corresponding observations made at Belgrade by Mr Math. Ivanowitsch, apothecary; and I have also obtained a great deal of interesting information from being able to converse with the Turks, as well as the Servians and Bulgarians.

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* We remark with pleasure the interest excited in Germany by the series of papers by Dr Boué which have appeared in this Journal. The following observations accompany a copious abstract of the first portion of our friend's discoveries, published by Mr Berghaus in his interesting and deservedly popular "Almanach; den Freunden der Erdkunde gewidmet," for 1838: "We may, with justice and propriety, term Boué's journey through European Turkey a journey of discovery for Geography as well as for Geognosy. For, although our maps of Turkey are filled up with the most minutely delineated chains of mountains, and exhibit a perfectly complete hydrographical net of serpentine rivers and streams; yet, we know well, that these apparently accurate representations belong, for the most part, to the phantasmagorical class, and can scarcely deceive the most credulous. Most of the other countries of Europe have been surveyed and described, but we grope in profound darkness, when we inquire into the natural external form and the geognostical constitution of the Turkish possessions. Christian prejudices and mercantile interests are undoubtedly the chief reasons that have, to so great an extent, prevented Europeans from travelling in a country which, now that many portions of it enjoy the blessings of peace, and that the early fanaticism of its inhabitants has begun to disappear, does not present the great difficulties formerly encountered. The indefatigable Boué, who has examined the geognostical structure of nearly all the countries of Europe, and who saw in Turkey an entirely unexplored field for new observations, resolved to devote from three to four years to its investigation, and to associate with him in his enterprize, naturalists who prosecuted other departments of natural history. In the year 1836, during a portion of his journey, he enjoyed the society of two French geologists, MM. Montalembert and Viquenel, of Mr Friedriehsthal, a botanist, and of Mr Adolf Schwab, a zoologist, the two latter being originally from Moravia. It is to be regretted that Boué did not take with him a measuring geographer, that is, a person, provided with the requisite instruments, who should have been able to determine positions, and to ascertain the three co-ordinates of a great number of points, so as to furnish a foundation for a new and accurate map, of whose want, our traveller, as we shall soon see, bitterly complains. . . . . . These are the facts collected by Boué, during the first year of his journey of discovery, so far as he has made them known in Jameson's excellent Journal. The services

To the north of the *central plateau of Mæsia*, between Pristina and Sophia, the limits of Servia are formed from E. to W. by the Jastrebacz (or chain of the sparrow-hawks), the Plocsa, and the Kopaonik. The first-named mountains are only covered by oaks, and higher up by elms, like the hills in central Servia; but these latter have fir trees near their summits. The greatest elevation of this chain appears to be rather above 5500 feet. The Jastrebacz is a mass of crystalline (primary) slaty rocks, whilst the others consist of transition-slates, with syenite, diallage-rock, serpentine, and metalliferous deposits, such as magnetic iron-ore, &c. *South of Mæsia Superior* is the Orbelus, if we may be allowed to apply this ancient name to the *Kurbetska-Planina*, a pretty large group of hills, situated at the sources of the Bistritza, and having an absolute height of between 4000 and 5000 feet. To the west and east of the Orbelus, there are some pretty low chains clothed with oaks, and forming the northern frontier of Macedonia, from Uskup to Dubnitza and Sophia. These hills are chiefly composed of talcose or micaceous slates, and scarcely attain an elevation of more than 3000 feet: they are sometimes still lower to the west of Kostendil, where they partly consist of transition-limestone, and even tertiary molasse. The Orbelus is a massive mountain of grani-

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rendered by this distinguished geologist to the knowledge of so large a portion of Europe, do not require my feeble praise; I wish him health, spirit, and perseverance for the continuation of his great undertaking." We have only to add, that we heartily subscribe to the opinions here expressed, by so competent an authority as Berghaus; and that we cordially unite with him in best wishes for the future success and prosperity of our traveller.—Edit.

tic rocks with gneiss; and in its vicinity we find trachytes, which are in connexion with those of Karatova. Trachyte also extends across the low central chain, from Strazin in Macedonia, to the vicinity of Vranja south of the Morava. A sulphureous hot spring issues at the northern extremity of these hills.

On the *eastern limits of Mœsia Superior*, south of Nisha, are situated the lofty *Stari Planina* (old mountain), and the *Suvo-Planina* (dry hill); these limestone ridges occur next to the mica-slates of Baditschka-Gora (east of Leskovatz). A very extensive group of mica-slate and talc-slate hills rises more to the south, between the Morava valley and the valleys of Trn and Sukova. On the north-east base of this group we find some trachytes and trachytic conglomerates; but, on the highly inclined southern declivity, porphyry dykes occur in the slates; whilst some of the hill tops are composed of trachyte and a white trachytic aggregate. Here, as in the Servian hills, elms cover the low *plateaux*, oaks the sides, and alpine pasturages occupy the highest summits, the chief of which is the broad *Snegpol* (Snow-field). This last is a little higher than the *Stari-Planina*, and attains an elevation of nearly 4000 (Paris) feet.

The *Snegpol* is united by the ridges above the village of Klissura (defile), to the *Kurbetska Planina* and the hills of Egri Palanka. The talc-slates, of which these are formed, are often decomposed, and contain numerous microscopic crystals of magnetic iron-ore, which is washed and smelted in many places. These hills completely separate the upper Morava valley from that of Trn, whose stream flows into the Sukova and Nissava; a fact of which most geographers are ignorant, as they make it fall into the Morava.

To the south-east of this group of hills we find lower ridges composed of limestone and newer transition-slate, or Silurian rocks, with numerous defiles or rents running nearly N–S. These hills extend to the great longitudinal channel which leads from Nisha to Sophia, and which is excavated, particularly towards the east, in a conchiferous limestone, probably belonging to the Jura formation. All the above-mentioned chains are inhabited by a pretty dense population of industrious Bulgarians.

Lastly, to the *west of the plateau of Mœsia* lies the Pristina or Kossova plain, surrounded by low chains, which are elevated about 800 or 1000 feet above it, whilst its own absolute height amounts to 1400 feet. The hills are chiefly composed of talc or mica slates, together with some serpentines and amygdaloidal limestones: they are covered with forests, chiefly of oak, of which the principal varieties are *Quercus Robur*, *Q. Cerris*, and *Q. pubescens*.

The middle plateau of Mœsia is occupied by hills a little higher than the preceding, and containing valleys or basins formed by the Morava, the Toplitza, and their defiles. Here the Bulgarians cultivate their fields and gardens in a most admirable manner. The valleys are covered with villages, where maps only indicate a wilderness, and the cultivated fields extend far up among the hills. Unfortunately, however, their neighbours the Arnautes, or Albanians inhabiting the N.W. of Mæsia superior, do not profit by their good example, but leave much of their ground in its primitive state of woody wilderness. Vines do not grow well in the Morava valley, except around Nisha, Leskovatz, Vranja, Prekop (Urkup), and also near Pristina. Indian corn is cultivated in the lower Bulgarian and Albanian valleys. The high mountainous ridges of Servia serve as a protection against the north winds.

The Morava valley is composed of tertiary beds of an argillaceous or sandy nature, as near Nisha and Leskovatz: alluvial beds occur higher up, in the Vranja basin. Nisha, Leskovatz, and Vranja are the chief towns of the three basins, of which the narrowest parts are at Kurvihan, and to the south of Leskovatz between that town and Surdebitza. Some trachytic eruptions have taken place to the south-east of Leskovatz, and siliceous limestone, probably of fresh water origin, occurs to the north-east, near Sheshiné, at the foot of the hills to the east of the Morava.

Between Radomir (at an elevation 1614 feet on the eastern side of the Strymon), Bresnik, and Sophia, is a very extensive *plateau*, nine miles broad, and composed of tertiary *augite porphyry*. The higher summits of the hills attain an altitude of 2456 feet. To the south rises the mountain called *Wistoska* or *Wistosh*, with its limestones, slaty and granitic rocks, argentiferous ores, and abundant springs. Its height may be estimated at above 4000 feet. To the west it overtops the bare hilly country around Radomir, and to the east the beautiful plain of Sophia, which has only an elevation of 1348 feet.

The great Isker (*Gomela or beuk Isker*) flows through the basin of that name, enters into the mountain defiles of Sumughu Balkan, and only leaves the hilly country six or nine miles to the south of Wratza near Butunia. Geographers have confounded in a singular manner the great Isker with the little Isker (*Malo or Kutschuk Isker*). This last river comes down the Balkan north of the Ichtiman basin, and joins the great Isker not far from Staro Celo. Etropol, Strigl (the Striga of maps), Tashkisi, and Komartzi, are on the little Isker, and not on the great, as marked in the maps, in which also the bed of the little Isker between Etropol and Starocelo is erroneously delineated as a part of that of the great Isker. The great Isker valley is the chief military highway from Bulgaria to Servia, but that of the little Isker only leads to a pretty high ridge of hills, which must be crossed before descending to the Sophia basin or to that of the Ichtiman, which is situated a little higher up, at an elevation of 1480 feet. The small Isker does not join the Sophia basin.

The hills of Southern Mæsia are united geographically to the Despotodagh, the Rhodopus, and the Balkan or Hæmus by means of the *Wistoska*, together with some granitic and sienitic hills at the base of the Rhodopus, and three or four low ridges running west 2° south to east 2° north, in an oblique direction from Banja to Ichtiman, between the Rhodopus and the Hæmus. These last are composed of mica-slate, gneiss, and granite, with some granular limestones, and their height is from 2000 to 2356 feet. At the base of the Despotodagh they are crossed by the *Kiz Derbend* (defile of the girl) running W.-E., and composed at its narrowest part of granular limestone. The road from Tatar Basardschik to Banja is carried along it. We must take care not to confound this defile with one of the same name, but much finer, in the Rhodopus between Rasluk and Newrokop. In the latter there are the ruins of an ancient castle, situated on high rocks, which overhang an old road a thousand feet above the torrent. The passage of this deep defile occupies nine hours, and all around appears a chaos. At Somakov the alluvial soil is full of microscopic particles of magnetic iron-ore; there are several founderies, and the Pasha of Sophia has built one after an English plan.

The *Balkan* or Hæmus extends from Sophia to Cape Eminek, and runs W. 3°, N. to E. 3°, S. or, by compass, nearly W.NW., E.SE. It is a much lower chain than the Despotodagh; the southern slopes are generally very steep, but, on the northern side, it is only the highest ridge which presents considerable inclinations. The Balkan is almost destitute of subordinate chains towards the south; and is composed of the principal high ridge, and a series of parallel lower ones which diminish in height as they approach the Danube. Between these are large longitudinal valleys; and we occasionally find rents from north to south intersecting the ridges, and occupied, as in the Alps, by the great rivers that issue from the longitudinal valleys. As the chain diminishes in height from west to east, the high Balkan (Kodja Balkan) forms the western part, at the sources of the Osma, where the summits probably attain an elevation of above 4000 feet; whilst, near the Black Sea, they have only a height of from 1800 to 2000 feet. There is an oblique and pretty high ridge to the west of Czatak and Bashkoë, which separates the waters of the Bebrova from those of the Akali-Komtschik. The eastern part of the Balkan is pretty well delineated in the Austrian map of Turkey, published at Vienna.

The high Balkan is composed of the crystalline slaty rocks, gneiss, mica-slate, talc and clay slates; these extend beyond Tschipka on the southern side, but their breadth diminishes from west to east. Above Islivné (which we find wrong placed and under the name of Selimno on maps), near the middle of the Balkan, there are some very picturesque hills of quartziferous porphyry; and among these the peaks of the *Tschataldagh* (rent hill) rise to the height of 2800 feet, thus affording a most beautiful view of Romelia. Islivné is a most delightful station for a naturalist: it is a Turkish town with 15,000 inhabitants, and has, along with Usundschova, the greatest annual fair in Romelia. I may remark that Tschirmen, near Usundschova, is on the southern and not on the northern side of the Maritza river.

Immediately above these older rocks of the Balkan we find a thick *formation of green sand*, composed of marly greyish sandstones, quartzose and greenish-coloured sandstones, and beds of marly clay and whitish, greyish, or black compact limestone. The latter rock is often conchiferous as at Wikrar, where some of the beds contain oysters, pectens, naticæ, turbinated shells, cariophylleæ, and other coralline bodies. In some cases orbitolites occur in great numbers, as near Loftdscha. The limestone forms thick beds, and occasionally craggy precipices; but the other rocks, with the exception of some of the green sandstones, only form hills, which are generally either covered with oaks and elms, or, being destitute of trees, are used as pasture grounds. The beds of the lowest cretaceous formation are not exactly parallel to the general direction of the Balkan, but appear to intersect it at a very acute angle, deviating a little to the south. The general dip is nearly N.NE.; but the undulations of certain masses often causes the dip to change to the S.W., N.W., &c.

At the eastern extremity of the Balkan, the green-sand is covered by extensive *plateaux* of chalk, with flints and belemnites. Chalk appears on all sides when it is not covered by the vegetation. Schumla is a good place for seeing the transition from the uppermost green-sand to the chloritic chalk with its fossils, the Grypheæ (*G. auricularis*, *G. vesiculosa*), Inoceramus (*I. sulcatus*), Pecten, Lima, Terebratula, Cucullea, Nerina, Natica, Cellepora, Flustra, Galerites, &c.; and also the transition from this last to the chalk with its characteristic fossils, the striated terebratulæ, shark's teeth, &c. The beds around Schumla are in an anticlinal position, and the town occupies the centre of the convexity or rent. By the aid of a little imagination it might be regarded as a crater of elevation, in which are deposits of tertiary clay, forming towards the east the natural double ramparts in the semicircular form of the town. On the other side it is inclosed by chalk hills, upon which a citadel and redoubts have been erected, in order to prevent the approach of enemies from the plateau. These chalk hills have been delineated by geographers as advanced portions of the Balkans; but certainly they ought not to convert mere small hills into chains of mountains.

If one might be allowed to judge from the inclined position of the Molasse near Islivné, it would appear that the *uprising of the Hæmus* was posterior to the deposition of the superior tertiary rocks; whilst the horizontal beds of tertiary sand on the central *plateau* of Mæsia would indicate an older epoch of formation for the hills of that country.

In the vicinity of the Danube, Bulgaria is covered with a *great tertiary formation*, which becomes broader as we proceed from east to west, in consequence of the oblique direction of the cretaceous beds in Bulgaria. This direction was owing to their having been deposited upon the jurassic limestone of the Pashaliks of Widdin and Nisha, and on the crystalline slaty rocks of the Balkan. The Danube flows past a series of small hills on the Bulgarian side; but, on the northern or Wallachian side, the country is flat. A great part of north-east Bulgaria and the country of the Dobrutscha Cossacks is tertiary. It is only near Babadagh, and between it and Matochin, that we find higher hills composed of older formations, particularly clay-slate. Löss or alluvial clay-marl occurs along the Danube. Erratic blocks are unknown in Bulgaria, as is the case with the other portions of Turkey which I have examined.

The isthmus between Rassova and Kostendsche, where a canal might be cut in order to shorten the voyage of the Danube, is not only occupied by alluvial matter, but also by some very low tertiary hills; the Danube can never have had its channel there in historical times.

South of the Balkan there exists only one pretty distant subordinate chain, viz., the low chain of transition-slate and limestone between Kalofer (west of Kezanlik), Eski Sagra, and a place west of Islivné, where the Tondja issues through a defile from its superior alluvial basin. This basin is the plain which extends between Tschipka, Kezanlik, and Czirkua, and it is on it that the roses are cultivated for making the attar of roses. The Vienna map delineates the course of the Tondja pretty well; but in Cotta's map it is very ill laid down.

From Islivné to Burgas on the Black Sea, there is a vast hollow at the base of the Balkan; the rivers are only separated by very small ridges. The remainder of the Balkan bounds the western part of the great tertiary gulf, whose surface forms the present soil of a great portion of eastern Romelia or Thracia.

The southern base of the Hæmus is remarkable for the exuberance of its vegetation, consisting of gardens of roses, jasmine, and wild lilac, vineyards and forests of all kinds of fruit trees, but without the olive tree, the *Anis* or the *Lepleb* (a kind of *Dolichos lablab*). Tschipka, Eski-Sagra, and Islivné, are truly delightful abodes. The adjacent plain, however, is destitute of trees, and consists chiefly of fields under cultivation, and pasture-grounds which are partly marshy, with a black soil. This alluvial and tertiary plain extends to the chain which runs along the Black Sea, as also the Sea of Marmora and the Archipelago. On its elevated borders near Eski-Sagra, there are some small deposits of fresh-water limestone. Primary alluvial matter fills up the angle of the basin between the Rhodopus and the Balkan, and it is on this spot that rice is chiefly cultivated. In other parts, the shores of this tertiary gulf have been covered by fossiliferous and coralline limestones, clay, sand, and sandstones; as along the sea of Marmora at Constantinople, around Kirklisse, between that town and Bunar-Hissar, between Serai, Tschataltscha, and Tschorlu, &c. The reddish-brown *smectic clay*, from which the Turkish pipes are manufactured without being exposed to the fire, also belongs to the tertiary formation, and is probably derived from granitic or recent igneous rocks. This clay occurs in the basin of Adrianople, as well as near Rutschuk in Bulgaria, near Sophia, at Komavitza near Nisha, &c. The best pipes are made at Belgrade, Nisha, Sophia, Rutschuk, Adrianople, Lule-Burgas, and Silivri.

In the *Plain north of Adrianople*, we occasionally meet with small groups of isolated hills composed of trachyte, as between Jeni-Sagra and Janboli, and near Karabunar. In the north-east portion of the tertiary basin of Romelia, the augite porphyry forms a very extensive undulating *plateau;* extending from the base of the Balkan, north of Aidos, to the Gulf of Burgas, and in a southerly direction to a different Karabunar (*black fountain*) situated on the Curu. In the wild and wooded country between Aidos and Burgas, there is a hot sulphureous spring, which is probably connected in some way with the focus of the eruptions, which formerly occurred in an ancient gulf between the Balkan and the *chain on the southern shores of the Black Sea.*

This last chain is not in immediate connection with the Hæmus; but, speaking geographically, is only a continuation of the chain at Eski Sagra. The great valley of the Tondja separates the low mountains north of Jeni-Sagra from the *plateaux*, composed of slates and blackish limestone, between Karabunar and Fakhi. To the south-east, the slates have been broken through by great masses of diorite and granite; and this latter, along with gneiss, predominates between Petschiomale and Kirklisse, and even farther to the east. The diorites chiefly occur in the neighbourhood of Fakhi, and seem to be veins in the granite. North of Kirklisse, the decomposition of the granitic rocks has produced fantastic shapes, and given rise to the formation of rocking stones, &c. On approaching the Bosphorus, the shore-chain is divided into a number of small hills situated on low-lying *plateaux*. Near Serai, we find clay-slates which extend to the Bosphorus, and alternate with greywacke and silurian shell-limestone, as near Therapia, and at the Giant's Mountain in Asia. To the north of Bujukdere there is a small trachytic district.

Geographers call my shore-chain *Strandsia Balkan* and *Kutschuk Balkan;* but the first name is unknown in the country, and is only that of a village south-east of Serai; whilst the second is a general denomination given by the Turks to all small hills. This chain is marked on maps as uniting with the Balkan between Karnabat and Islivné; but, at the distance of only three miles east from Islivné, the waters, issuing from the wooded valleys of the Balkan, flow towards the Black Sea. A channel could be cut from Islivné to Burgas. Islivné is near the point where the waters flow in opposite directions, to the Archipelago and the Black Sea, indeed it is only separated by a small hill from the waters which flow in an easterly direction.

The Rhodopus does not extend to the Dardanelles as all maps indicate. Like the Perindagh, the mountains above Rasluk, and those of the famous Kiz-Derbend, its highest summits are situated towards the west, viz. to the south of Banja, Samakov, and Dubnitza. These attain an elevation of 8000 feet, and perhaps some of those which I did not ascend are still higher. The chain gradually diminishes in height from west to east, and terminates rather abruptly about five leagues from the Maritza, and six leagues south-west of Adrianople. The declivities on the northern side are generally pretty steep; and magnificent views of this portion of the Rhodopus are obtained from Philippopolis and Banja. Pine trees occur highest up on the hills, next to them are elms, and lower down, the oak forests. A certain number of rents running north-south form deep valleys, which are adorned with monasteries (Stanimak) and villages or cottages. These fissures are the passes by which people cross the mighty wall from Philoppopolis, Tatar-Basardschik, Banja, Samakov, and Dubnitza. Along the Archipelago, the lofty crags, the island of Tassos with its marbles, as well as the high Semedrek, all indicate that the crystalline slates, the gneiss, the granite, and the granular limestone of the Despotodagh or Rhodopus have experienced considerable depressions, as, otherwise, the chain would be united to the old ridges of the Troja country and the Ida. Some isolated portions of the Rhodopus are found in the northern tertiary plain, as, for instance, the hills of gneiss and granite between Harinani and Haskoë. At Philippopolis, *sienite* forms four small hillocks in the town, or close to it: this formation is connected with the granitic eruptions from west to east at the base of the Rhodopus, to the west and east of Samakov, and at Dubnitza.

From the Archipelago to beyond Karabunar (south of Dimotika) there is a long stripe running south-north of *trachyte* and *trachytic conglomerate.* Here one finds all the compact, semivitrified, and vitreous varieties of these igneous masses which made their appearance during the tertiary epoch. These eruptions must be connected on the one side with those north of Adrianople, and on the other with those of the high island of Semedrek. In this latter, as also to the east of Fered, there is a hot sulphureous spring. On the northern base of the Rhodopus, hornblendic trachytes are found in some parts of the Semisdsche valley. The trachytic country is partly a stony barren soil, and partly covered with low trees of the *Paliurus aculeatus.* It is only fertile where the conglomerates are in connexion with the tertiary argillaceous-calcareous beds.

South of Edrené (Adrianople), between the Maritza and the Dardanelles, are the low ridges and *plateaux* of the *Tekirdagh.*

In these, Molasse is associated with clay and a sand which occasionally contains shells, together with numerous fragments of siliceous coniferous (?) wood. One bed, at the height of 800 feet, is chiefly composed of shells of the genus *mactra.* Coralline and shelly limestones are found on the sands of Malgara, and particularly on the western banks of the Maritza, near the trachytic zone around Fered. The greatest height of the Tekirdagh may amount to nearly 900 feet. To the south-east of Aimadtschik, there is a somewhat more elevated ridge, which, in the vicinity of the Sea of Marmora, is probably 300 or 400 feet higher than the Tekirdagh. It is improperly called *Kagridagh* in the Vienna map.

It would appear that, during the epoch of the deposition of the middle and superior tertiary rocks, the sea did not pass through the Dardanelles, as this rent did not then exist; but that the great sea of Marmora and Adrianople communicated with the Archipelago by means of a vast firth situated more to the west, and of which the deepest part is now occupied by the large and fertile valley of the Maritza. Near Constantinople also, the tertiary limestones and superior sands do not occur beyond the mountains of greywacke and slate south of the Black Sea, although it is probable that tertiary waters existed in the cavities between the groups of the chain running along the Black Sea. The rents of the Dardanelles and the Bosphorus were formed at a later period, during the alluvial epoch; as appears from the craggy declivities of their banks, the correspondence of the beds on both sides, and the absence of any very old alluvium. I do not suppose that Andreossy's opinion is supported by any geologist, as there are no traces of fluviatile or marine erosion on the banks of these straits.

To the west of the central *plateau* of Mæsia is *Upper Albania*, a country occupied to the east and south-east by the high *Tschar*, (not Tschardagh, which is an Asiatic hill on the Sea of Marmora), between Kacsanik, Kalkandel, and Prisrend; and to the south by its prolongation, consisting of the high ridges interposed between the wild primary valley of the Debres and the plain of Bitoglia (Monastir) and Perlepe, as also the valley of Kalkandel, called Tetovo in the Bulgarian language. They are sometimes more than 8000 feet high, and are occasionally capped with snow during summer. These ridges are immense masses of crystalline slates, which, in the Tschar, become talcose or argillaceous, and contain whole hills of compact or semi-granular limestone. As an example I may mention the high and massive Taleshdagh, the Galitza of maps, nearly 6000 feet in height, at the eastern entrance of the black Drin. The hills behind Prisrend and its old Servian castle are also calcareous, and abound in fine springs. The roads through the Tschar from Prisrend to Kalkandel, and from Prisrend to Kacsanik, are very interesting from the beautiful views which they command—valleys with lofty crags, ruins of castles and monasteries, and villages scattered among high declivities.

To the west of these ridges, and rising to an average height of 6000 or 7000 feet, are the primary mountains of Elbessan, through which passes the only military road from Romelia or Monastir to Scutari. It is carried through Ochri, where the vineyards indicate that the elevation of the lake of Ochrida is under 2000 feet; it may, however, be estimated at 1500 or 1600 feet. More to the south we meet with the northern extremities of the primary Pindus with parallel limestone ridges like those of the Tschar.

To the north of these chains, all of which, with the exception of the Tschar, run from north-west to south-east, there have been immense eruptions of *Diorite*, compact *Euphotide* and *Serpentine*, and among these we find all the compact, lamellar, sienitic, decomposed, and earthy varieties of the ophite of the Pyrenees. *Diallage-rock* occurs rarely, but forms some magnificent masses and small hills in this deposit around the torrent of Rapé, thirty miles east of Scutari. These igneous rocks contain considerable masses of clay-slate, &c., through which they have been upheaved, and which they have altered and indurated in various ways. Whole rocks and mountains are composed of red and greenish jaspers, and these pass into the slate from which they have been formed, as in Italy, by igneous agency. This singularly intersected and barren district includes all the mountains between the confluence of the black and white Drins, and the place where they issue from the calcareous rent six miles from Scutari, comprehending a tract six miles from west to east, and nearly fifteen miles from north to south. The height of these hills does not seem to be above 3300 feet; some of their summits are covered with pines and fir trees, as is particularly the case with the *Kiafa-mala* (hill of Kiafa), near Vlet. The coniferous trees descend to the elevation of 1500 feet, and the forests lower down are composed of elms and oaks.

This country, which bears some resemblance to a sea agitated by a storm, is the native place of the *Myrdites* (not Myrmides), a race of Catholic Albanians who extend from Scutari to beyond Takova, and from the Debres to Prisrend and Alessio. Their captain, Doda, resides at Oros; and he can assemble together at least 10,000 armed men. These poor people dispute with the torrents the ground necessary for cultivating their Turkish corn; and they conceal their villages as much as possible from their enemies the Turks. The deep rents in which the Drin flows through the dioritic and calcareous hills are seldom well adapted for roads, but one has been made between Prisrend and Scutari, along three valleys and two mountain defiles running nearly west-east. Old pavements and bridges shew that this road had been used in ancient times; but, although it is crowded with horses carrying goods from Trieste into Turkey, it is at present only a footpath, and scarcely practicable for riding. This road abounds in precipices, steep ascents and descents, flights of steps cut in the rock (called *Skale* in Albanian), and passes excavated in the rock and too narrow for horses. It is ninety-six miles in length, and during the whole distance, the traveller meets with scarcely a single village, although a dozen isolated inns indicate at least as many hidden villages at a distance from the road.

These dioritic hills are connected geographically with the high and calcareous ridges of the lower Drin, which are only the extremities of the very high similar chains between Ipek or Scherkoles and the country of Montenegro. Their greyish-white summits covered with eternal snow, except towards the east, and their numerous rents, recall to one's mind the secondary calcareous chains of the Alps. Around the lake of Plava, from which the Bojana issues, near Gusinie above Plava, and a little to the east of that, their elevation probably exceeds 8000 feet. Their lower declivities are covered with villages; for the Albanian, like the inhabitant of Montenegro, seeks liberty and exemption from taxes in the wildest abodes; whilst the Turk bestows on him the epithet of *Haiduk*, rebel and highway robber.

These serrated ridges are connected with the Kom and Dormitor, two high chains to the east and west of Drobniak (the Drobnasche of maps), as also with the Lubitschnia; all of which are limestone hills capped with snow, and at least as high as the former ones. Farther north, these chains are united to those east of Glubigne, between Mostar and Novaschin in Herzegowina. It is the *Bielosok* of the Vienna map.

The maps place Plava and the neighbouring snowy chains too far from the Drin and Scutari, and too much to the east; besides, the Albanian names are ill spelt, and there are numerous errors in the position of inns and villages between Prisrend and Scutari.

It is possible that the eruptions of diorite and serpentine, in the country called *myrdita*, may be contemporaneous with one of the latest upheavings of the chains last mentioned; and that Bosnia received, during that epoch, its present form, which may be compared to a roof sloping from south to north.

The *cretaceous formation*, with hippurites, compact limestone, and marly sandstone, &c., occurs in the superior tertiary basin of the White Drin, north and east of Takova; but this basin, which is at an elevation of 1000 or 1100 feet, is separated from the alluvium of Scutari by a very thick wall of diorite, serpentine, and limestone; and from this the Drin issues with great velocity at Scale, at six miles east of Scutari. The inferior chalk is found on the coasts of Albania, where it occurs, as in southern Europe, in the form of singularly shaped hills of limestone, which is much more frequently compact, like that of the Jura, than fossiliferous or earthy. This chalk is distributed over Dalmatia and part of Herzegowina, and forms numerous high hills around the bay of Cattaro. It extends to Montenegro, where its rocks are so numerous as materially to impede the cultivation of the country; and the superabundant population make use of this as a pretext for mitting robberies on their neighbours, whether Mahome- Catholics, or even Servians of the Greek Church. These robberies are the more horrible, from the circumstance that the robbers always carry away with them the heads of their victims, in order to serve as a trophy and obtain a remuneration from their bishop or chief. This latter personage often pays reluctantly, and offers remonstrances; but all to no purpose, as his power is limited. The Pasha of Scutari is also obliged to reward those who bring him heads from Montenegro, a thing which happens almost every day. The only vineyards in Montenegro are those near the Lake of Scutari, along the Moratscha (Moracca of maps), and near Bielopavltzi.

The cretaceous system forms all the hills around Scutari; the lake of which contains some rocky islands of limestone. The Scutari basin is so much protected from the northern winds, that the heat is very oppressive during summer; but the climate is, on this account, favourable for the growth of Mediterranean plants, such as the pomegranate and orange tree. The olive tree also occurs there; but its true native country commences more to the south at Durazzo. Owing to the quantity of stagnant water, fevers are common in Scutari during summer.

At Scale, on the Drin, the cretaceous limestone is compact, apparently without fossils, and of a yellow or greyish white colour. Similar characters are observable in the limestone forming the hills east of Scutari. At the base of these hills there are numerous villages and vineyards, whose green tint forms a strong contrast to the grey barren rocks. To the east of Alessio, the same limestone forms conical hills like those of Antivari, north of Scutari. More to the south, they extend east of Durazzo to Berat. The acroceraunian or chimerian chain, with its sandstones, belongs to the same formation, which probably crosses over to the Ionian isles, and along the coast to Prevesa. These mountains exhibit frequent examples of natural fires, similar to that of the *pietra mala* in the Appenines. The limestone ridges around Janina appear to be older, and analogous to those of the Pindus and the Tschar.

Large springs, issuing like rivers out of the rock, occur in this formation, as in the older limestone districts. In Montenegro, the Czernojevich issues as a river from the chalk at the distance of one and a half leagues north of the Lake of Scutari.

Cettigne is situated in a dry valley, and the only torrents are those near Genognussi; but the waters quickly disappear under ground, a fact of which geographers are not aware. On the other hand, to the east of Ipek, the Istok issues as a torrent from a subterranean channel in the old limestone; and the White Drin makes its appearance as a river from under a limestone crag at the distance of one and a half miles above Novo Celo, six miles east of Ipek. Geographers, induced probably by the similarity of names, have marked this Drin as having its origin at Rosalia, a village on the Bosnian side of the chain, and on one of the higher branches of the Drina in Bosnia. Besides, the position of Rosalia is wrong; as it is too much to the west, or Ipek too much to the east. The high plateau of Nikshichi (Niskiki of maps), in Herzegowina, has no lakes, but only caverns or *katavotrons*, in which the water loses itself, and which may occasionally overflow after long rains.

Bosnia is an immense *plateau* inclined from south to north, and presenting to the Albanian plain of Ipek, and the hills of Myrdita, a limestone wall from 6000 to 7000 feet high. It requires four or five hours to ascend to the *plateau*. To the west, Bosnia is bounded in a similar manner by the snowy chain east of Mostar, and the Kom west of Kolaschin and east of Drobniak. Thus Moştar is another Nice, protected towards the north, and surrounded with gardens of pomegranate, olive, and orange trees, rising in terraces one above another,—a fertile oasis in the stony calcareous soil of Herzegowina. Towards Servia, the *plateau* of Bosnia also descends very abruptly, especially to the north-east; but to the south-east it is connected with the Servian hills near Uschitze, and on the banks of the Ibar.

We may travel for many miles on level ground on the highest and most southerly *plateau* of Bosnia: occasionally we meet with small valleys or small basins, the sites of former lakes, as the plain north of Ugrlo. From this, the most elevated *plateau*, all the ridges diminish in height towards the north, and run S.E.–N.W., so that they form a slightly inclined plain so far as the Save. The highest ridges run parallel to the Kom and Dormitor, &c., in Herzegowina, but have only half or two-thirds the altitude of these mountains, or of those near Plava. They diminish in height in a direction from west to east. Geographers are right in marking an oblique chain, higher than the others, between Bosnia and Herzegowina. If we imagine, in this inclined surface of the ridges, deep furrows running, some S.E.–N.W., or from south to north, and others from west to east, we have a configuration similar to that of Bosnia, with its chains running S.E.–N.W., or S.SE.–N.NW. These last, however, anastomose by lower ramifications running W.–E.; so that the small plains and valleys, where towns or villages are situated, are inclosed by walls running S.E.–N.W., and by low ridges running W.–E. Here, as in the Alps and the Balkan, rents running N.–S. give issue to the great rivers, which thus find their way out of the longitudinal valleys.

This natural citadel is only connected with Turkey by the broken part of the high walls above Novibazar (*Jenibazar*) and Ipek; and with Scutari or Maritime Albania by two or three mountain-passes which are still higher, and are rendered dangerous owing to the snows above the Plava Lake, and the precipices which occur for nine miles on the bank of the Zem. These last passes lead to Zienitza and Bielopol. If Montenegro were a quiet country, the Turks, when going to Bosnia, might ascend the Moracca river, or cross Montenegro and the high plains of Herzegowina.

Bosnia, that beautiful Servian-Mahometan Switzerland, is a cold country, whose hills are covered with firs, pine trees, and birches. The plants chiefly cultivated are, barley, oats, *Polygonum fagopyrum*, rye, potatoes, hemp, flax, and plum trees for making brandy. Turkish corn does not grow well, except in the deep sheltered valleys, as north of Sexajevo (Bosna Serai); and vineyards only occur near the banks of the Save, at Brod, and at Banjaluka, &c. On the southern higher ridges of Bosnia there is, below the forest of fir and pine trees, a zone of elms, which rises to the height of 3800 feet on the southern declivity; oaks occur lower down, and, on the borders of the Albanian low country, there are chestnut trees and vineyards.

Bosnia, like Central and Western Servia and a part of Croatia, is entirely composed of my *primary formations* (the *tran-*

sition ones of authors), and particularly of the older and medial divisions. The chief rocks are clay-slates, of a grey or red colour, greywacke, conglomerate, compact limestone, sometimes intersected by spathose veins and reddish or whitish sandstones (Pratza, &c.). The immense calcareous masses form lofty and picturesque crags, as along the Verbas, south of Banjaluka, and between Serajevo and Pratza. In other places they form subalpine valleys of great beauty, as that of the Lirn, near Priepol, that of Millscheda or Miloscheveda, between Zienitza and Priepol, &c. We occasionally find deep calcareous defiles on the banks of the great rivers, as on the Drina south of Zwornik, on the Jadar near the castle of Kizlar, and on the Bosna. The Gothic castles of Hissar (south of Priepol), Kizlar, Vranduk, Zwornik, &c. are situated on the tops of rocks or on craggy walls in these defiles. The calcareous *plateaux* offer many small funnel-shaped hollows, resembling the *combes* of the Jura. The limestone is often fossiliferous, containing encrinites, terebratulæ, pectens, &c.; and it is probable that many Silurian fossils will be discovered in it.

The slates of Bosnia are intersected, as in Western Servia, by masses of sienite, sienitic porphyry, and felspar porphyry, as in the Vrt valley, near Tschainitza. Serpentine and *Schaalstein* occur near Kizlar and in other places. In the vicinity of these igneous rocks, and especially near the first mentioned, are the silver ores of Crebernitza. The gold of Slatibor (hill of gold), near Uschitze, has probably a similar position. There are rich iron mines in limestone at Maidan, in the neighbourhood of Banjaluka, and at Foinitza, near Serajevo; and in these, sparry iron and brown iron-ores occur. The chief founderies are near Bosna-Serai, from whence a great quantity of iron goods are sent into Turkey. It is said that gold was formerly obtained by washing the sand on the Bosnia.

The *compact slightly fossiliferous limestone of the hills and plateaux near Albania*, distinctly alternates in certain localities with varieties of clay-slate, and even with reddish coloured sandstones, as at Mount Glieb. It would, therefore, be advisable for the present, to class these beds, or at least those limestones which are at a distance from Montenegro, among the newer transition-formations. Future travellers will be able to fix the exact limits between these and the Mediterranean chalk, and also to decide on the presence or absence of Jurassic rocks; as there will be much less difficulty in conducting these investigations, when the country becomes more cultivated, and the inhabitants more civilized.

The *tertiary formation* is not found in the great hollows of the *plateau* of Bosnia; it only occurs towards the north, as a border of low hills, occasionally covered with inconsiderable forests of oak. These hills form a distinct separation between the higher and older chains, and the great plains, south of the Save, which abound in oak forests. They can be traced from Schabatz, in Servia, to Losnitza, and thence to Brod and Dubitza, on the upper Save. In Servia, on the contrary, the tertiary hills stretch far up in the great valleys as from Schabatz to Vallievo, and on the eastern bank of the Kolubara. They consist of the same rocks as in Hungary, clay and sand alternating with shelly or coralline limestone, which is often of a white colour; in short of the medial and superior divisions of the tertiary series. Near the Save there is *löss* or alluvial calcareous clay; and we find, deposited on its banks, numerous trees which have been floated down by the rivers in Bosnia.

The mineral waters, noticed by me this year (1837), were chiefly hot sulphureous springs, as near Aidos, on the Tondja, nine miles east of Kezanlik, at Sophia; at Banja, on the base of the Rhodopus; at the curious *Batak-banese* (boggy bath), occurring in granite, half a league north-east of Banja; at Fered; and at Banja, six miles east of Vranja. I must add to these, the acidulous seltzer water of Jarmazow, near Bosna-Serai; and those of Lepenitza, between Bosna-Serai and Zwornik. In Romelia, there are acidulous ferruginous springs at Berki, near Jeni Sagra; and at Hasskoë, east of Adrianople; but as the Turks do not sufficiently value this latter kind of springs, the traveller finds more difficulty in obtaining information about them than about the hot springs, which are always converted into bathing places by the Turks.

The temperature of the thermal springs is various. At Aidos it was 33° R.; at Banja, near the Rhodopus, 45° and 46°; at Sophia, 34° and 35°. However, notwithstanding this heat, the water near the Tondja contains *Confervæ*. The chemical composition of these waters seems to be always nearly the same. In particular, they all contain sulphate of soda; and some, as those of Sophia, a little sulphate of magnesia. They all indicate the presence of more or less sulphuretted-hydrogen in a free state. Others contain a little muriate of magnesia or lime, as those of Banja, Aidos, and Sophia. I was unable to detect the presence of carbonic acid or iron. Sometimes, but rarely, there are very minute traces of lime, as at Banja.

Temperature of Cold Springs.—At Alexinitza, in the low hills of Southern Servia, a spring gave 13½° to 14° cent., the temperature of the air being 17°; at Jasen, in the lower part of the hills south-east of Leskovatz in Mœsia, a spring was at 12°, whilst the air was at 17° in the shade, and at 26° in the sun; a fountain on the *plateau* of the neighbouring hills, at an elevation of 900 feet, gave 16°, the air being at 19°; near Trn a spring was at 10°, and the air in the shade at 23°; at Bresnik one was at 10°, another at 11°, the air being at 28°, and a third at 15°, the air being at 18°; in the plain of Sophia a spring was at 13°, the air in the shade at 18°; at Sopot, near Lofdtscha in Bulgaria, at 12¾°, and the air at 19°; at Csatak, in a valley of the Balkan, at 13½°, the air being at 23° in the shade, and 26° in the sun during the month of June; at Karasholi, near Osmanbazar, at 11°, the air being at 22° in the shade, and 31° in the sun; at Schumla at 12°, the air at 18° in the shade; at Rodosto (Tekirdagh), on the Sea of Marmora, at 18°, the air being at 26°; on the Tekirdagh *plateau*, south of Rodosto at an elevation of 700 feet, at 15½°, the air being at 18°; at Malgara, south of the Tekirdagh, at 13°, the air being at 21°; near Banja, at the base of the Rhodopus, at 13°, the air being at 21°; at six miles west of Prisrend, a spring in the limestone was at 12° or 13°, the air being at 28° in the month of August; at Scutari in Albania, the water of a deep well gave 12°, the air being at 29°; at Mokro, in the low hills, not far from Bosna-Serai in Bosnia, and at an elevation of 2700 feet, a spring was at 12°, the air being at 8° during September.

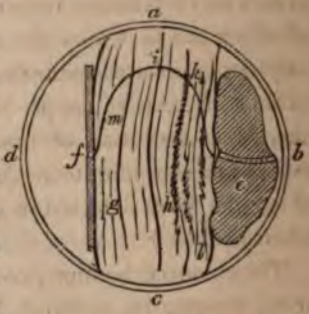
These are the chief facts collected during my travels in 1837. Geographers may doubt some of my positions, and it is possible [illegible] may have committed some errors; but I had this in my [illegible] that I was able to converse with the natives of each [illegible] in their own languages, and that, in addition to a strong desire for examining new countries, I always enjoyed good health during my travels. Travellers in Turkey are now secure and comfortable if they know and follow the customs of the country, and if they are accompanied by a Tartar. The Sultan has put an end to all highway robberies by means of the gibbet, his *Nisam* or European army, and the stations of *gendarmes*. Although some *Haiduks* still exist near the frontiers, they are only rebels, called highway robbers by the Turks; and, although the Turks dread them, the European traveller has nothing to fear from them, as they expect much from the interest which European Christians take in their situation. Indeed, people of all ranks are well inclined towards strangers. In Albania, the Christian, especially the *Nemse*, German or Austrian, finds security in places where the Turks are in danger; for the Myrdites are Catholics, and the Mahometan and Greek Albanians hate the Turks. In Bosnia strangers are well received, even during the petty wars which too often devastate that country.

The only exceptions to this rule are some countries where only Asiatic Turks reside, and where there are Turks of the good olden time with their green turbans; as also a few Mahometan Albanian villages. Yet, even in these, the traveller is not exposed to any annoyance, but only experiences indifference and neglect. The former haughty Turkish appearance has now disappeared very much, although scarcely any of the Turks, except those employed in the civil and military departments, have thought it worth while all at once to change their handsome dress and turban for our European fashions. Besides, our dress requires household furniture, which does not exist in Turkey. No Turk seems as yet to have adopted our dress-coat. In places where Turks only reside, one still often finds that noble hospitality of the middle ages which our inns have banished from Europe. Turkey has undergone great reforms, but it must yet submit to many other changes. May a stable, just, and impartial government, soon procure for the inhabitants that security of person and property which travellers at present enjoy; and may the plague, and the injurious distinctions of *Raja* and *Ghiaours*, soon disappear from Turkey. By results such as these, rather than by the creation of an European army, the Sultan's name would be rendered immortal.

On the Lamination of Clay by Electricity. By R. W. Fox, Esq., F.G.S. &c.

Some clay was exhibited to the Polytechnical Society of Cornwall by R. W. Fox, Esq., which had become *laminated* by long continued voltaic action, so as to resemble clay-slate in its structure.

The figure here given may serve to illustrate the process by which this was accomplished. Let *a*, *b*, *c*, *d*, represent the top or rim of an earthenware cup or basin; *e*, a piece of copper pyrites; *f*, the upper edge of a plate of zinc; *i*, copper wire by which the two latter were connected; and *g*, *h*, the top of a mass or wall of clay between the copper-ore and zinc, and forming for each of them, a water-tight cell. The cell containing the copper-ore was filled with a metallic solution,—the sulphate of zinc, for instance,—and the other with water mixed with a little sulphuric acid. The water with which the clay was worked up was also acidulated. Thus circumstanced, the apparatus was set aside for three or four months, and was not disturbed till some little time after the water had evaporated, and the clay had become perfectly dry throughout.



It then exhibited, on breaking off a portion of its upper part, lines of cleavage of a schistose character, parallel to the sides of the clay and plate of zinc, or, at least, as nearly so as was consistent with their undulatory form. In other words, the lines or laminæ were at right angles to the direction of the electrical forces.

They are indicated by the lines on *g*, *h*; and the strongly marked line *a c* represents a principal line of division which separated the clay into two portions, from the top to the bottom.

These seemed to form, as it were, two voltaic plates in opposite states of electricity, and one of them consequently, more favourable than the other for the reception of metallic deposits and other bases from their solutions. These results, which, however, were much more marked in some experiments than in others, seem to confirm the correctness of the views stated in the last Polytechnic Report, pp. 87, 88.*

Indeed, the general laminated structure of the clay appears to indicate, that a series of voltaic poles were produced throughout the clay, the symmetrical arrangement of which had a corresponding effect on the structure of the clay. This view is still more strikingly confirmed by the occurrence, in several instances, of veins, or rather laminæ of oxide of iron, the edges of which are shewn by the shaded lines *k*, *l*, *m*. In these cases sulphate of iron was substituted for the sulphate of zinc; and laminæ of oxide of copper were sometimes formed in like manner, when a solution of that metal was employed; and, moreover, numerous minute insulated portions or specks of the oxide of copper were detected in different parts of the mass of clay when broken.†

The coloured laminæ produced in some of these experiments, have, apparently, a direct bearing on the variously coloured and waving lines which the edges of the laminæ of schistose rocks so commonly present, and also, on the occasional deposition of metalliferous substances parallel to the cleavage, which seem to have been formed on or before the consolidation of the rocks. The true veins, however, were evidently formed after the consolidation of the latter; and as they generally intersect, in Cornwall at least, the laminæ of the "killas" or clay-slate, at more or less considerable angles, they seem to have acted the part of the copper wire shewn at *i*, in completing the voltaic circuit.

Hence arises the question whether the productiveness of lodes, depends more or less on their bearings with respect to to those of the cleavage; and it appears to be a matter of practical importance to ascertain these relations in our mining districts, and especially to note the angles at which the laminæ are intersected in their horizontal bearing and underlie, by lodes which yield given ores in the greatest abundance.*

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* We have not seen the last Report mentioned above. Edit.

† Some of these effects were apparently most decided when a series of voltaic exciters were used so as to increase the energy of the action,—three or four such cups for instance as shewn in the Fig. arranged so as to form a complete circuit.

Proceedings of the Wernerian Natural History Society.—Continued from vol. xxiv. p. 428.

March 24.—Dr CHARLES ANDERSON, V. P. in the Chair.—Dr William Macdonald read a paper on the Analogy between the Locomotive Organs in Fishes and Insects, illustrating the theory of unity of organization throughout the animal kingdom, with demonstrations from specimens, accompanied by drawings and diagrams. Dr R. Hamilton exhibited additional drawings, by Mr Stewart, of the various species of the seal tribe, and made observations on the subject. Professor Jameson exhibited an extensive collection of Fossil Fishes found in the limestone of Burdiehouse, belonging to R. J. Hay Cunninghame, Esq., and Mr Cunninghame expressed his dissent from some of the opinions maintained by Dr Hibbert regarding these remains.

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* The following is the report of the Royal Cornwall Polytechnic Society on the above interesting experiment of Mr Fox :—

" Of objects of speculative and experimental science, your Committee have to notice a communication from Mr R. W. Fox, which, if his views be established, involves the most important conclusions. We refer to the specimens of clay, exhibited by that gentleman, which, after having been submitted, in a moistened state, to weak voltaic action for some months, were found, when dry, to be distinctly laminated, having precisely the appearance of clay-slate. It is well known that this laminated structure is common to many rocks, which are proved, by the organic remains which they contain, to be of sedimentary origin. It is also known that the direction of the laminæ or cleavage, with respect to the stratification of any given rock, differs exceedingly in different places, these being at various angles with regard to each other. Hence it follows, that this remarkable structure cannot be referred to slow deposition, or to any mechanical causes. Neither does such complete independence of the stratification and cleavage of rocks in reference to each other, seem to accord with the definite character and tendency of the phenomena of crystallization, to say nothing of the chemical objection to mechanical matter assuming a crystalline form, without undergoing fusion or solution. Mr Fox's discovery that electricity is capable of producing this structure in clay, seems, in his opinion, to meet the difficulties of the question; and he considers the prevailing directions of the electrical forces, depending often on local causes, to have determined that of the cleavage, and the more or less heterogeneous nature of the rock, to have modified the extent of their influence."—*Fifth Annual Report of the Royal Cornwall Polytechnic Society*, pp. 20, 21.

April 7.—Professor JAMESON, P., in the Chair.—Mr Hay Cunninghame read a Geognostical Account of the southern part of the mainland of Shetland, exhibiting specimens of the different rocks, and illustrating his descriptions by large coloured sketches of some of the more interesting junctions, veins, &c.

April 21.—Professor JAMESON, P. in the Chair.—Mr Smith of Jordanhill read a paper on the Latest Changes of the Level of the Sea, particularly in the Basin of the Clyde, and exhibited a series of shells from different elevated beaches. Mr Torrie read Dr Lawrence Edmonstone's observations on the Distinctions, History, and Hunting of Seals in the Shetland Islands. He then gave a brief notice of Dr Boué's account of the geology of some parts of European Turkey (published in our present number); and communicated an abstract of Mr John Lawson junior's observations on the geology of the Lower District of Moray, with a description of various mineral deposits in the vicinity of Elgin. There were laid on the table, 1. A series of daily observations on the Thermometer, Hygrometer, Barometer, and Rain-Gauge, made at the Manse of Abbey St Bathan's, by the Rev. John Wallace. 2. A Comparative Register of the Sympiesometer and Marine Barometer, kept in the Hon. E. I. Company's Ship Charles Grant, during a voyage from England to Bombay in 1836, by Henry Graham, Esq.—The Society adjourned till November next.

Proceedings of the Botanical Society.

February 8. 1838.—Professor GRAHAM, P. in the Chair. The following members were elected; *Resident*, Mr Graham Craig, Mr John Shaw, Mr John Sinclair, Mr W. B. D. D. Turnbull; *Non-Resident*, Mr Robert Maulkin Lingwood, B.A. Christ's College, Cambridge.—Specimens were presented from ten members of the Society, received since 11th January.

A letter from Dr Tyacke was read, containing an account of a botanical excursion in the Spring of 1837 to the Channel Islands and the coast of France, with remarks on several of the species collected.

Observations by Dr Graham on plants collected in Scotland, in 1837, by Dr M'Nab were read. He noticed particularly the following:—*Arenaria norvegica;* first seen on Serpentine hills, to the northward of Balta Sound, Shetland, by a son of Dr Edmonton, and afterwards found by Dr M'Nab in the same place. Specimens collected by Dr Pollexfen in 1835 were shewn to the Society.—*Cerastium latifolium, var.* with dense, cespitose habit, orbicular leaves, profuse glandular pubescence, and straight cylindrical capsule, scarcely longer than the calyx. *Hab.* Shetland.—*Lychnis dioica, var.* with pale rose-coloured flowers, and stem rarely three inches high. Seen by Mr James M'Nab some years ago, and found to retain its peculiar habit in cultivation. *Hab.* near Newton-Stewart, Galloway.—*Agrostis canina var.* is perhaps *Trichodium alpinum* or *rupestre.* Dr Graham thinks the absence of the inner valve of the perianth, though not a generic, is a good specific character; plant first noticed by Dr Graham in Sutherlandshire, some years ago, and afterwards by Mr W. M'Nab, in a viviparous state in the same county. *Hab.* on the top of Goatfel, Arran. —*Fedia mixta, Vahl;* specimens were gathered along with this, shewing the transition from *F. dentata.* *Hab.* near Whithorn.

Mr R. W. Falconer read a paper containing an account of the most celebrated gardens of antiquity, with observations on the Hortulan taste which they exhibit. After some introductory remarks upon the probable origin of gardens, he proceeded to give a detailed account of the gardens of Alcinous mentioned by Homer; the Hanging Gardens of Babylon; the parks or gardens of the Persians, mentioned by Xenophon; the gardens of Daphne, in Syria, and the gardens of the Hesperides. He then gave an account of the gardens celebrated by the ancient Greeks and Romans; among the latter, those of Lucullus at Baiae, of Pliny at Tusculum, and Laurentum. Mr Falconer considers that although a taste for gardening evidently prevailed to some extent among the ancients, yet that it never attained to any perfection except among modern nations. Flowers, he also believes, never constituted a peculiar feature of ancient gardens, and that they were not esteemed as objects of taste by the ancients, who appear to have cultivated them only as decorations to be employed on occasions of public and private rejoicings.

Mr James Macaulay then read a paper, the object of which was to prove that flowers were esteemed by the ancients as objects of taste, and cultivated as a source of amusement. He argued that the very fact of flowers being deemed worthy of being offered to the gods proved a previous taste and value for them; and gave examples of gardens amongst the ancient Hebrews, Greeks, and Oriental nations, where *amœnitas*, and not *utilitas* alone, must have been the object in the cultivation of flowers. He next alluded to the gardens mentioned in the Latin classics, and contended that the garden of Lucullus, so often referred to, ought not to be regarded as a specimen either of the art or the taste of his time, as it was censured by his own contemporaries, Cicero and Varro; the latter expressly stating "*Hortos Luculli non floribus fructibusque sed tabulis fuisse insignes.*" He also shewed, on the authority of Horace, Martial, and Pliny, that the citizens of Rome used to cultivate plants on the balconies of their houses, and to rear flowers in boxes and in flower-pots, which were called "*horti imaginarii;*" and that it is not likely the rich would do this merely to procure materials for their votive offerings, or to supply the ornaments for their entertainments, but that a taste for the cultivation of flowers as objects of amusement must also have prevailed.

March 8.—Dr BALFOUR, V. P. in the Chair.—The following members were elected:—*Non-Resident*—the Honourable Louisa Anne Neville of Audley End, near Saffron Walden. *Foreign*—M. A. Fischer, Basle; the Reverend Louis Leresch, St Cierge, sur Moudon; M. Luhler, Wirtemberg; M. Otto Wilh. Sonder, Kiel, Holstein. Specimens were presented from eleven members of the Society, received since 8th February. Letters were read from Dr Von Martius and Professor Ledebour on their election as honorary members.

Dr Graham read the continuation of his observations on the plants collected in Scotland in 1837 by Dr M'Nab.—*Erythræa littoralis.* Dr Graham thinks it doubtful whether there is more than one British species of *Erythræa;* and if the present is to be considered distinct, that its only character would seem to rest on the narrow linear segments of the five-partite calyx, equal in length to the tube of the corolla. *Hab.* Brodick, Arran.—*Lathyrus maritimus*, is apparently the plant of the North of Europe, of Canada, and of the United States as far south as Boston, and may be easily distinguished from *L. pisiformis* of Ledebour, or the figure of Gmelin quoted by him, and in Hooker's British Flora, by the winged stem and the shape of the stipules. The variety which Dr Graham considers to be the type of the species is distinguished by its compact robust growth, and by the common petioles being much arched backwards; whereas the present plant is of a slender somewhat straggling habit, not from growing in wooded ground, but probably from being the inhabitant of the less genial climate to which the species is extended. It appears not to differ from *Lathyrus venosus* of American botanists. *Hab.* sands on the shore at Barra Firth, Unst, Shetland, where Dr Edmonstone had observed it for several years.—*Ervum tetraspermum*, and *Allium arenarium*. *Hab.* near

Kirkcudbright.—*Cladium mariscus.* *Hab.* Ravenstone Loch, Whithorn.—*Lamium intermedium.* *Hab.* Shetland.

Mr Campbell read a communication from Colonel P. J. Brown of Eichenbühl near Thun, containing a sketch of the botany of the neighbourhood of the Lake of Thun, Switzerland, chiefly in reference to the geographical distribution and altitude of the species enumerated. The Lake of Thun having an elevation of about 1900 feet above the sea, and the surrounding country being much intersected by hills or long ridges, the vegetation assumes a subalpine character on the pastures about 1800 feet above the lake, comprising *Trollius Europæus*, *Hieracium aureum*, *Tussilago alpina*, *&c.* The following is given as an approximation to the species usually met with at different heights on the surrounding mountains:—Between 2000 and 3000 feet, *Arenaria verna* and *ciliata*, *Dryas octopetala*, *Cotoneaster vulgaris*, *Hieracium villosum*, *&c.* Between 3000 and 4000 feet, *Silene acaulis*, *Cerastium alpinum*, *Phaca astragalina*, *Oxytropis uralensis*, *Saxifraga oppositifolia*, *Hieracium aurantiacum*, *Arbutus alpina*, *Ajuga alpina*, *Orchis pallens*, *Carex atrata*, *&c.* Above 4000 feet, *Gnaphalium alpinum* and *Leontopodium*, *Petrocallis pyrenaica*, *Draba tomentosa*, and *stellata*, *Androsace bryoides*, *&c.* Colonel Brown concludes his paper by stating that he hopes to be able to communicate fuller information as to the precise elevation of the different localities mentioned, on some future occasion.

SCIENTIFIC INTELLIGENCE.

GEOLOGY.

1. *Subterranean Heat.*—Bischof's explanation of the higher temperature that prevails in the depths of the ocean, the hypothesis of Babbage and Herschel on the heating of the solid parts of the earth, the views of Keilhau, Fox and others, on subterranean and submarine action, are all tending to the evolution of important geological principles.

2. *Subterranean Temperatures.*—M. Walferdin has communicated a notice to the Academy of Sciences of Paris, on a pit sunk by M. Mulot at St André (*département de l'Eure*), and on observations of temperature made in that pit, at a depth of 830 English feet. The sinking has been carried to a depth of 862 feet, without any spouting spring being met with. The

following is the series of substances traversed, together with their thickness:

	Ft.	In.
Plastic clay, . .	44	5
White chalk, . .	400	6
Chalk marl, . .	95	9
Glauconite, . .	45	4
Green sand, . .	276	9
	862	10

M. Walferdin made an observation on the temperature at a depth of 830 feet in this pit on the 18th of June last. Two of his *thermomètres à déversement* were sent down, each enclosed in a glass tube, sealed by the lamp at its two extremities; and after a period of ten hours, the one was found to indicate 64°.32 F., and the other 64°.27 F. The mean temperature of the plateau of St André being unknown, M. Walferdin has taken as his point of departure the temperature of the only pit existing in the *commune*, which he has found to be 53°.96 F. at a depth of 246 feet. By calculating, according to these data, the increase of temperature with the depth, we find it to be 1°.8 F. for every 101 feet 6.55 inches. M. Walferdin compares this result with those obtained previously from observations made in the pit sunk at Grenelle, and in that of the Military School, adopting as a point of departure the constant temperature (53°.24) of the cellars of the observatory, at a depth of 91 feet 10 inches. Two experiments, made at different times in the pit of Grenelle, at a depth of 1312 feet 4.31 inches, give for 1°.8 F. 103 feet 3.17 inches, and 101 feet 3.39 inches. In the pit at the Military School (also sunk in chalk), and distant about 1968 feet from the pit of Grenelle, at a depth of 567 feet 7 inches, the temperature was found to be 61°.52 F., thus giving for 1°.8 F. 101 feet 2.6 inches. It thus results from observations made at various depths of from 567 feet to 1312 feet, that the rate according to which the temperature increases with the depth *in the chalk formation*, appears to be regular in the Paris basin. It would be important to ascertain, by experiments made with care, if, in the middle and lower parts of the secondary formations, the temperature increases with the depth at the same rate; and M. Walferdin now proposes to direct his attention to this point.—(*Comptes Rendus*, 16*th Avril* 1838.)

3. *Temperature of the Earth.*—The following observations were made by Dr Magnus with his *Geothermometer*, on the temperature of a bore sunk by M. C. v. Wulffen, at Pitzpuhl, near Burg, about nine English miles from Magdeburg:—

At a depth of	150	feet, the temperature was	49°.77 F.
....	200	....	50.67
....	250	....	51.8
....	300	....	53.15
....	350	....	54.61
....	400	....	55.62
....	457	....	56.63

The bore was provided with iron tubes to the depth of 427 feet; but when the observations were made, the portion below the tubes had already become so filled up with mud, that it was impossible to cause the thermometer to descend farther than 457 feet. The increase of temperature in this bore was pretty regularly 2°.25 F. for every 100 feet. The deepest observation was at a point more than 200 feet below the level of the sea; for the place where the bore begins lies 111 feet above the level of the *Pegel* near Magdeburgh, which itself is about the same height as Berlin, whose elevation above the Baltic has been lately determined to be 108°.5 Rhenish feet.—(Poggendorff's Annalen, vol. xl. p. 145.)

4. *Extract of a Letter from M. Erman junior to M. Arago, upon the Temperature of the Ground in Siberia.*—I hope you will look at those parts of my historical journal which treat of the climate of Northern Asia with some interest; and in relation to this subject, I beg to direct your attention to the 242d and following pages. I have there given the result of the data obtained regarding the climate of the town of Jakouzk. The depth of a well which M. Schergin, a merchant in the town, had then excavated to the depth of 50 feet (English), in the hope of finally reaching strata which were not frozen, and which would be capable of supplying water, was always, when I made trial, at the temperature of — 6° R. equal to 18°.5 Fahr. The temperature of the surface of the soil should not at the time have exceeded this degree of cold, since the latitude of the place was 62° 1′ 29″. This result appeared to me eminently paradoxical; but I have since confirmed it, by calculating ob-

servations made on the temperature of the air in the same town during many consecutive years, with thermometers which I have carefully compared with my own. Some results are subjoined:

Mean Month of		6 A. M.	2 P. M.	9 P. M.
January,	.	—32°.8 F.	—30°.77 F.	—31°.9 F.
February,	.	—44.50	—36.4	—41.8
March, .	.	—17.27	+ 1.63	— 7.82
April, .	.	+ 8.15	+30.43	+17.15
May, .	.	+35.82	+47.30	+37.62
June, .	.	+54.27	+67.77	+53.60
July, .	.	+64.40	+79.70	+61.70
August, .	.	+57.22	+72.72	+58.32
September,	.	+38.75	+50.00	+40.55
October,	.	+11.30	+21.20	+13.33
November,	.	—13.45	— 9.17	—12.77
December,	.	—42.92	—39.77	—42.02

You will conclude from these observations, as I have in the accompanying volume, that *the mean temperature at Jakoukz is perfectly in accordance with the temperature of the upper strata, which I have observed, by taking my thermometer to a depth of 50 feet (English) below the surface.* This being the case, it necessarily follows, that, in boring deeper, unfrozen strata will not be reached till the increase of heat resulting from the approach to the centre of the globe shall amount to 6° R., equal to 45½° Fahr. The experiments which have hitherto been made in the pits of Europe, and those which I have made in the Oural mines, carry this increase to 1° R. = 2°.25 F. for every 90 or 100 French = 96 or 106 English feet. Hence, I do not expect the unfreezing at Jakouzk at a less depth than 500 or 600 French feet = 533 or 639 English feet. The observations which M. Schergin has made since my departure from Jakouzk, and during which they have descended to a depth of 400 English feet, perfectly confirm what I have advanced concerning the mean temperature of the air and soil of this locality; for they have since found at

the depth of	77	feet English, a temperature of	+ 19°.63 Fahr.
....	119	....	+ 23.00
....	382	....	+ 30.88

They also indicate, for the strata occurring in this country, an increase of heat in the ratio of 1° R. = 2°.25 F. to about

60 feet English; that is to say, a much more rapid augmentation than has been observed elsewhere. The only method, as it occurs to me, in which we can explain this phenomenon, is by attributing to the upper strata over Northern Asia a greater conducting power of heat than the other parts of the globe which we inhabit; and this result will be the more striking, as it comes in some degree to support another result of the same kind. In fact, the excessive variations of temperature which have been observed at Jakouzk, and in other parts of Easter Siberia, during the course of the solar year, lead us to the conclusion that the earth's surface is there endowed with a radiating and thermal power much superior to that of Europe.—*Comptes Rendus*, 16th Avril 1838.

5. *Metamorphic Rocks.*—That doctrine of the Edinburgh Plutonian School, first taught by Hutton, and now known under the name of *mineral metamorphism*, is rapidly progressing. Its chief supporters are our countryman Lyell, and on the Continent, Keerstein, Virlet, Boblaye, Keilhau, Von Buch, Beaumont, &c. &c.

6. *On the Formation of Serpentine.*—Mr Böbert, in Keilhau's Gaea Norvegica, states, as the conclusions he has drawn from an attentive examination of the various local portions of serpentine occurring near Modum in Norway, that many serpentine masses owe their existence to the transformation of other minerals; that the general diffusion of serpentinic substances, with all their variations, may chiefly arise from the impulse to their production and formation having been communicated at a very remote epoch under relations and conditions unknown to us; and finally, that some serpentines are only to be regarded as the middle link in the conversion which several minerals undergo into the kind of talc called *Speckstein.* Mr Böbert states, that in the serpentines of Modum there are neither traces of Neptunian deposition, nor of volcanic action.

7. *Beryl of Aberdeenshire.*—Dr Fleming informs us that it occurs in contemporaneous veins of coarse granite in gneiss at the Stonyhill of Nigg. It has been known as occasionally occurring in similar veins in the granite of Rubislaw, where, in the rock itself, some small tolerably perfect crystals may occasionally be met with.

8. ***Reopening of the Manganese Mine at Grandholm, near Aberdeen.***—We are informed by Dr Fleming that the ore occurs in a rock of mica-slate, which stretches in a northerly direction, and has an easterly dip, varying from 30° to 50° and upwards. In some places it is thin slaty, even, or waved, while at other parts thin beds of gneiss and granite make their appearance. When the mine was opened upwards of twenty years ago, an excavation, or *opencast*, was made across the stretch of the strata, at the eastern termination of which a mass of felspar porphyry makes its appearance, the relations of which are not seen at present. The ore, which is the "grey Manganese ore," or "Hydrous Binoxide of Manganese," occurs in irregular thin beds, rounded concretions or anastomosing films in the rock, accompanied by small quantities of sulphate of barytes. As the working has but recently commenced, little more than a dozen of tons of the ore have been obtained. But as the undertaking is in hands possessing abundance of capital and enterprise, Messrs Cookson of Newcastle, the mine will now have a fair trial, and it is hoped that a new branch of trade will thus be added to those already so successfully carried on at Aberdeen.

9. *On the Formation of Calcareous Spar and Arragonite.*—M. Gustav Rose draws the following conclusions from a series of experiments performed by him on this subject:—1. That in the humid way both calcareous spar and arragonite are formed, the former at a lower, the latter at a higher temperature; but in the dry way calcareous spar only is formed. 2. That the carbonate of lime, immediately after precipitation from a cold solution, is in an indistinctly crystalline condition, which resembles that of chalk, and from which the distinctly crystalline condition afterwards proceeds. 3. That arragonite can be very easily converted into calcareous spar, in the moist way, by allowing the arragonite obtained by precipitation to stand in water or in a solution of carbonate of ammonia; in the dry way, by exposing the arragonite to a low red heat, when the large crystals fall into a coarse powder, but the small crystals retain their form and produce pseudomorphous crystals. It results, moreover, from the investigations of M. G. Rose, that the origin of arragonite cannot now be ascribed, as has

often been the case, to the small quantity of carbonate of strontia which natural arragonite for the most part contains. This, indeed, is evident from the fact, that there is arragonite which does not contain any carbonate of strontia; but it is completely proved by our being able to produce with ease artificial arragonite, which contains no strontia whatever. A solution of chloride of calcium was purposely mixed with a small quantity of a solution of chloride of strontium, but by precipitation at the common temperature with carbonate of ammonia, crystals of calcareous spar only were observable.

10. *Fall of Meteoric Stones in Brazil.*—On the 11th December 1836, about half-past 11 o'clock P. M., with a clear sky, and S.W. wind, a fire-ball of uncommon size and brilliancy, appeared over the village of Macoa, at the entrance of the river Assu; it immediately burst with a loud crackling noise, and a shower of stones fell within a circle of ten leagues. They fell through several houses, and buried themselves some feet deep in the sand, but they did not occasion any further damage than killing and maiming a few oxen. The weight of those picked up varied from one to eighty pounds.—*Poggendorf's Annalen*, No. 12, 1837.

11. *Intermittent Spring.*—In a letter to M. Rozet, Dr Boué gives a short account of the Miracle Spring, in the district of Bihar in Hungary, in the lordship of Bressa, on the eastern side of the commune of Mabugyer. It issues from the foot of a mountain, loses itself among the stones, and proceeds in the form of a torrent to join the Moros. After a hollow noise it suddenly gives out, several times a day, large quantities of water, so as to fill the bed of the stream in two minutes. The flux is more frequent from Christmas to the middle of summer, and during that period it takes place every quarter of an hour; but is more rare during the last half of the summer and in autumn, when nevertheless the weather is more moist.

ZOOLOGY.

12. *A Memoir upon Microscopic Animalculæ, considered as a Cause of Putrefaction*, has lately been presented to the French Academy of Sciences, by MM. Beauperthuy and Adet de Roseville, and the results of their investigations are announced by the authors in the following terms. 1st, When any animal sub-

stance is put into circumstances which favour putresence, there is observed after a certain time, which varies according to temperature and moisture of the atmosphere, the formation therein of a number of *animalcules;* and that before any insipid or musty smell (marking the first period of the putrid fermentation) is perceptible; and even before the liquid presents any sign of an acid or alkaline state. These animalcules, which at first have the shape of *monades*, and then assume that of *vibrios*, derive their nourishment from the substance in which they are developed, and multiply in it with the greatest rapidity. 2d, At a more advanced period, when the liquid reddens litmus paper, the microscope shews us animalcules in immense numbers, and especially upon the brownish pellicle with which the surface of the liquid is covered. A considerable number of crystals are also seen to be mixed with the animalcules; and still there is no kind of unpleasant odour. 3d, Somewhat later the fluid is observed to be more and more charged with detached particles of the animal substance which had been plunged into it: all these particles are formed of agglomerated animalcules attached to some fragments of the decomposing tissue, and this is the first epoch at which an odour begins to exist, faint at first, but speedily putrid. 4th, In the fourth and last period the animalcules shew themselves in tens of thousands, and the time arrives in which the whole mass becomes completely organized, and consists of nothing else than these elementary beings. By this time the liquid has become alkaline, and is extremely fetid.—*Comptes Rendus*, 19 *Mars* 1838.

13. *Analogy between the organic structure and red colour of the globules in the blood of animals, and of those red vegetable globules named Protococcus kermesinus.*—In a memoir read by M. Turpin to the Academy of Sciences, on globules in animal fluids, we find the following observations:—What has just been stated regarding the presence of smaller red globules in the globules of the blood, is perfectly explained by the very analogous structure of those small red vegetables, globulous and vesicular, so generally distributed throughout nature, and which often tinge with a blood-red colour, the surface of calcareous rocks—the surface of water, both fresh and salt—snow and ice—the crystals of sea-salt—and, finally, as we pro-

ceed immediately to observe, the translucent and colourless substance of red agates; vegetables which are more particularly designated by the names of *Protococcus nivalis*, *Protococcus kermesinus*, Agardh, and *Hœmatococcus*, &c. These small vegetables, though larger by a half than the globules of the blood, still have with them a great analogy as it regards their organization, and probably also their chemical composition. A transparent and colourless vesicle (or perhaps two, the one included in the other) perfectly spherical, and filled with red and reproducing globules, forms the whole of the organization of these small vesicular vegetables, which, with some other analogous ones, mark the first efforts of organization, and seem to be nothing more than first attempts, or the representatives of the elementary or constituent organs of the cellular masses of more complex vegetables and animals. When the minute internal globules of these small vegetables begin to increase within the maternal vesicle, to become reproductive seminules, they cause the vesicle to assume very much the mammillated aspect of a strawberry. According to this mode of development, is it not probable that those blood-globules of animals which, on account of their shape, are called strawberry globules, are also produced by the increase of a certain number of the red globules which they contain? All my microscopic researches compel me not only to admit this analogy, but likewise to think that the red globula of the globules of the blood are the seminules of those organized bodies which are destined to replace, and sometimes to multiply, the old globules of the blood, as they become extinct and cease to live, as individuals, in the midst of the serum which serves as their habitation, and in which they procure their nourishment.

14. *Cause of the Red Colour of Agates.*—The red colour of agates is owing to a number, greater or smaller, of *Protococcus kermesinus* accumulated together, or more frequently reduced to their small red globules (seminules) agglomerated or coagulated, and distributed, according to certain circumstances, in the colourless structure of these siliceous compounds. Since we have now been considering, analogically, those innumerable *Protococcus kermesinus*, and the red globules they contain, I beg leave to add, that microscopic and comparative investigations

which I have recently made, and which I purpose to publish elsewhere in all their details, have clearly demonstrated, that the various colours, rose, orange, blood-red, and reddish-brown (varieties owing to more advanced growth), which are inclosed in, or which surround, the translucid and colourless structure of different kinds of agates, were found to be owing to the presence either of red globules uniformly mixed, as in the carnelian agate; or agglomerated into small irregular clots, and distributed into circular waves, according to certain forms or conditions which existed at the time of the siliceous conglomeration; or finally, though more rarely, to these small red vegetables themselves, quite entire, and perfectly distinctly visible with the microscope. It is impossible to find a resemblance in colour and polish more striking than that which is seen in a white glass phial filled with *Protococcus kermesinus*, when compared with a carnelian, as may be fully established by the trial. (*M. Turpin.*)

15. *On the Stinging Power possessed by some Medusæ.*—It appears that the belief in the stinging or burning power of the *Medusa aurita* is founded on a confusion of species. In the East or Baltic Sea, where this species alone is found, I have often, while bathing, brought my body in contact with the animal, without receiving any injury. Even touching it with the tongue caused no feeling of sharpness. On the contrary, I have suffered much in the North Sea, from violent burning and swelling of the back of the hands, caused by much contact of the hand with the red *Cyanea* (*Medusa*) *capillata*. In the North Sea, therefore, as at Wangeroge, Norderney, Heligoland, Cuxhaven, &c., bathers ought, as in the Mediterranean Sea and the Atlantic, to avoid contact with medusæ; while on the south coasts of the Baltic, as at Swinemünde, Doberan, &c., they have nothing to dread from these exquisitely beautiful creatures. It is, therefore, undoubtedly proper to warn bathers in other places than the Baltic, against coming in contact with any medusæ whatever, as it is an easy matter to confound the injurious with the harmless species. Even on the northern coasts of the Baltic, injurious medusæ are frequently to be met with after storms. They speedily die, however, as they require salter water; so that on

the south coast of the Baltic we only find the harmless, extremely delicate *Medusa aurita.*—(*Professor Ehrenberg in his paper; "Uber die Akalephen des Rothen Meeres und den Organismus der Medusen der Ostsee," published in the Transactions of the Berlin Academy of Sciences for* 1835. *Berlin,* 1837.)

16. *Phosphorescence of the Ocean.*—The naturalists of *La Bonite* in her late voyage round the globe, have made many observations respecting marine phosphorescence, which are thus reported to the *French Academy of Sciences.*—Many observations made upon phosphorescent water by means of reagents, of filtration, boiling, simple examination, and with the help of the microscope, have led us to the following conclusions. The phosphorescent property of sea-water is not inherent in the nature of this liquid, but is essentially owing to the presence of organized beings. The animals which produce the phosphorescence belong to different classes. In the first rank, we find the minute species of crustacea which swarm in the sea, but especially a very small species having two valves, which possess this remarkable property in the highest degree. All these species have been collected, and are carefully preserved in alcohol. Many mollusca, principally small *Cephalopodes pelagiens, Biphores* (Salpae), &c., and also many zoophytes, among which we remark *Diphyes*, Medusæ, &c., also possess the phosphorescent property. Finally, in certain localites, we also find on the surface of the ocean very small yellowish bodies which are nevertheless extremely phosphorescent. We have encountered these small bodies in immense abundance when landing at the Sandwich Isles, and in crossing from this archipelago to the Marianne Islands. We encountered them in such vast quantities at the Straits of Malacca and upon the coasts of Pulo-Penang, that the whole surface for a great extent appeared covered by a thick yellowish dust. These small bodies have been examined with the microscope; but although they have been for a long time submitted to our notice, we have never been able to detect the slightest movement connected with them. At the same time, the experiments we have made on them through the means of various reagents, lead us to the conclusion that they are organized and living bodies. They appeared some-

what different, as taken at the Sandwich Islands and in the Straits of Malacca. The former were globular and transparent, with a yellowish point in the centre, the latter were rather oval with a depression in the centre, so that they were somewhat kidney-shaped; they also were entirely yellowish.

In all the animals which possess phosphorescence, the property has appeared to us to depend upon a particular principle, probably a secretion of these animals, which, however, differs as to the manner in which it is scattered around. Some of them, as the small phosphorescent crustacea, can distinctly emit it in certain circumstances, especially when, by any cause, they are irritated; they then project true jets, regular *fusées* of phosphorescent matter, in such quantities as to form a luminous atmosphere in which they disappear. We have succeeded in collecting a certain quantity of this matter upon the sides of a vessel which contained a great number of these crustacea. Others of these animals did not appear to possess the power of emitting this matter, and in them it was developed only in certain circumstances, as for example, when they struck against any body, or when they moved, or when causes of irritation operated upon them. In others again, as in the Cephalopoda, and in some Pteropoda the phenomenon exhibited itself in a way that was nearly quite passive. The phosphorescent matter contained in their neucleus, or in other parts of their bodies, shone constantly and uniformly so long as the animal was in the enjoyment of life, and along with this disappeared the light they shed abroad. Finally, in the yellowish corpuscules above described, the phosphorescent matter also shines almost uniformly, but if brought into contact with any reagent, their lustre is first increased, and then insensibly vanishes away. The phosphorescent matter which we collected on the sides of the vase, was yellowish, slightly viscous, and very soluble in the water, which it rendered luminous at the moment it was projected by the animal.—*Comptes Rendus*, No. xv. 5 *Avril*, p. 458.

ARTS.

17. *On the Composition of a new Indelible Ink. By Dr Traill.*—In a paper lately read before the Royal Society of Edinburgh, Dr Traill, after an account of many unsuccessful expe-

riments to produce a durable ink from metallic combinations, stated that he was induced to attempt the composition of a carbonaceous liquid which should possess the qualities of good writing-ink. The inks used by the ancients were carbonaceous, and have admirably resisted the effects of time; but the author found that the specimens of writing on the Herculaneum and Egyptian *papyri* were effaced by washing with water; and on forming inks after the descriptions of Vitruvius, Dioscorides, and Pliny, he found that they did not flow freely from the pen, and did not resist water,—qualities essential to a good writing-ink in modern practice. The carbonaceous inks with resinous vehicles, rendered fluid by essential oils, though they resisted water and chemical agents, had the disadvantages of not flowing freely from the pen, and of spreading on the paper, so as to produce unseemly lines. Solutions of caoutchouc in coal-naphtha, and in a fragrant essential oil, lately imported from South America, under the name of *aceite de sassafras* (the natural produce of a supposed Laurus), were subject to the same objections. The author tried various animal and vegetable fluids as vehicles of the carbon, without obtaining the desired result, until he found, in A SOLUTION OF THE GLUTEN OF WHEAT IN PYROLIGNEOUS ACID, a fluid capable of readily uniting with carbon into an ink, possessing the qualities of a good, durable, writing ink. To prepare this ink, he directs gluten of wheat to be separated from the starch as completely as possible, by the usual process, and when recent to be dissolved in pyroligneous acid with the aid of heat. This forms a saponaceous fluid, which is to be tempered with water until the acid has the usual strength of vinegar. He grinds each ounce of this fluid with from eight to ten grains of the best lamp-black, and one and a half grain of indigo. The following are the qualities of this ink. 1. It is formed of cheap materials. 2. It is easily made, the colouring matter readily incorporating with the vehicle. 3. Its colour is good. 4. It flows freely from the pen. 5. It dries quickly. 6. When dry it is not removable by friction. 7. It is not affected by soaking in water. 8. Slips of paper written on by this ink have remained immersed in solutions of chemical agents, capable of immediately effacing or impairing common ink, for seventy-two hours, without change, unless the solutions be so concentrated as to injure the texture of the paper. The author offers this composition as a writing-ink, to be used on paper, for the drawing out of bills, deeds, wills, or wherever it is important to prevent the alteration of sums or signatures, as well as for handing down to posterity public records, in a less perishable material than common ink. He concluded his paper by stating, that should it be found to present an obstacle to the commission of crime—should it, even in a single instance, prevent the perpetration of an offence so injurious to society as the falsification of a public or a private document, the author will rejoice in the publication of his discovery, and consider that his labour has not been in vain.

NEW PUBLICATIONS.

1. *Gaea Norvegica.* Conducted by Professor KEILHAU. Part 1st. 4to, with Geological Map and Plates. Christiania, 1838. Dahl. (*In German*).

THE writings of Von Buch, Hausmann, Naumann, Esmark, Brongniart, Bedemar, and other travellers, and those of Professor Keilhau himself, have already put us in possession of a large mass of geological knowledge respecting Norway. A detailed and systematic geological description of that interesting country is, however, still a desideratum; and we hail with great gratification the long expected appearance of the first part of a publication which will furnish the materials for such a work. The "*Gaea Norvegica*," which may in some respects be compared to Dufresnoy and Beaumont's "*Memoires pour servir à une description géologique de la France*," is to appear from time to time, is to consist of memoirs by various individuals, and is to be conducted by Professor Keilhau, who will himself contribute by much the largest portion of the matter. That indefatigable and able geologist has examined almost every portion of his native country, has traversed it repeatedly in all directions, and has digested and prepared for publication nearly the whole of his observations. It is to be regretted that the future numbers of the *Gaea* cannot be expected to follow the present one with much rapidity or regularity, owing to the obstacles arising from the expense of the undertaking, the delay caused by the translation, and the time required for the preparation of the maps and plates, &c. The Royal Norwegian Society of Sciences at Drontheim has, with becoming liberality, given pecuniary aid to the publication. The first number is of itself a very valuable addition to our geological literature, and is chiefly composed of an excellent detailed account of the transition district of Christiania, by Professor Keilhau, which is accompanied by a well executed and minute geological map and good sections; but also contains a paper on the primitive serpentine of Modum, by Mr Böbert, inspector of the cobalt-mine at Modum; and a general view of the Norwegian trilobites (no less than forty-eight species), by Mr C. Boeck.

2. *Lethæa Geognostica; or Figures and Descriptions of the Characteristic Petrifactions of the different Rock-Formations.* By Dr G. H. Bronn. 4to. Heidelberg. 1837.

This excellent work, which we have already repeatedly noticed, is now nearly finished, one number only remaining unpublished. Its beauty, accuracy, utility, and cheapness will, we doubt not, procure it a place in every geological library.

3. *A Systematic and Stratigraphical Catalogue of the Fossil Fish in the Cabinets of Lord Cole and Sir Philip Grey Egerton, together with an Alphabetical and Stratigraphical Catalogue of the same Species, with references to their published figures and descriptions.* By Sir Philip Grey Egerton, Bart. M.P., F.R.S., F.G.S. 4to. London: 1837.

The student of palæontology will prize highly the very interesting tables in this unpretending catalogue; which, although extending over comparatively but few pages, contains much valuable and accurate information.

4. *The Zoology of the Voyage of H.M.S. Beagle, under the Command of Captain Fitzroy, during the years* 1832 *to* 1836. *Part 2d. Mammalia, with numerous Plates.* By George R. Waterhouse. Esq., Curator of the Museum of the Zoological Society. Smith, Elder & Co., London.

This part of the Zoology of the Beagle, contains a short but interesting geographical introduction by Mr Darwin, illustrative of the principal localities of the animals collected by him during the Fitzroy expedition. Mr Waterhouse figures beautifully, and describes well, three bats belonging to the families Phyllostomidæ, Vespertilionidæ, and Noctilionidæ. Of these the most interesting is the vampire bat. Of the family Carnivora, one species of canis, the Antarcticus, is described and figured; but the letter-press of the following characteristic coloured engravings, *Canis Magellanicus*, *C. fulvipes*, *C. Azaræ*, *Felis Yagouarondi*, *F. pajeros*, *and Delphinus Fitz-Royi*, is delayed till next number.

5. *The Natural History of the Quadrupeds of Paraguay and the River La Plata.* By DON FELIX DE AZARA. Translated from the Spanish, with numerous Notes, by W. P. HUNTER, Esq., F.G.S., Z.S., &c. Vol. 1. 8vo. Black & Co., Edinburgh; Longman & Co., London.

An English translation, from the Spanish, of even a part of the celebrated work of Azara, cannot but be considered as a valuable addition to the zoological treasures of our literature. We trust it is the precursor of the remaining volumes of the Natural History of Paraguay. Mr Hunter, who, we doubt not, will continue his interesting notes, ought to consult the writings of Renger and D'Orbigny on the animals of South America, works of great value, although but rarely met with in English libraries.

List of Patents granted in Scotland from 13*th March* 1838 *to* 11*th June* 1838 *inclusive.*

1. To **EUGENE RICHARD LADISLAS DE BREZA** of Paris, in the kingdom of France, now of St Martin's Street, Leicester Square, in the county of Middlesex, gentleman, for an invention of "a chemical combination or compound for rendering cloth, wood, paper, and other substances indestructible by fire, and also preserving them from the ravages of insects."—19th March 1838.

2. To **JOHN PATERSON REID**, power-loom manufacturer in Glasgow, and **THOMAS JOHNSON**, mechanic, in the employment of John and Archibald Reid, power-loom manufacturers there, for an invention of "certain improvements in preparing yarn or thread by machinery, suitable for warps in preparation for weaving on looms."—22d March 1838.

3. To **HENRY BESSEMER** of City Terrace, City Road, in the parish of Saint Luke, Old Street, and county of Middlesex, engineer, for an invention of "certain improvements in machinery or apparatus for casting printing types, spaces, and quadrats, and the means of breaking off and counting the same."—23d March 1838.

4. To **Richard Tappin Claridge**, late of Salisbury Street, Strand, but now of 8 Regent Street, gentleman, in consequence of a communication made to him from abroad, for an invention of a "mastic cement or composition applicable to paving and road-making, covering buildings, and the various purposes to which cement, mastic, lead, zinc, or composition, are employed."—27th March 1838.

5. To **Jeremiah Bynner** of Birmingham, in the county of Warwick, lamp-maker, for an invention of "improvements on lamps."—30th March 1838.

6. To **Augustus Coulon** of Tokenhouse-yard, in the city of London, merchant, partly in consequence of a communication made to him by a certain foreigner resident abroad, and partly by invention of his own, for an invention of "improvements applicable to block printing."—30th March 1838.

7. To **Joshua John Lloyd Margary** of Wellington Road, St John's Wood, in the county of Middlesex, Esquire, for an invention of "a new mode of preserving animal and vegetable substances from decay."—30th March 1838.

8. To **Julius Oliver**, late of Castle Street, Falcon Square, in the city of London, but now of Queen Street, Golden Square, in the county of Middlesex, gentleman, in consequence of a communication from a certain foreigner residing abroad, for an invention of "a certain improvement in the filters employed in sugar-refining."—6th April 1838.

9. To **Charles Wye Williams** of Liverpool, in the county of Lancaster, gentleman, for an invention of "certain improvements in the means of preparing the vegetable material of peat-moss or bog, so as to render it applicable to several useful purposes, and particularly for fuel."—6th April 1838.

10. To **Alexander Happey** of Basing Lane, in the city of London, gentleman, in consequence of a communication from a foreigner residing abroad, for an invention of "a new composition applicable to paving roads, streets, terraces, and other places, which improvements are also applicable to the different purposes of building, and also in the apparatus for making the said composition."—9th April 1838.

11. To **John Stewart** of Glasgow, in that part of the United Kingdom of Great Britain and Ireland called Scotland, for an invention of "improvements in machinery for manufacturing ropes, lines, twines, and yarns, from hemp, flax, or tow."—12th April 1838.

12. To **Marie Claudine Veronise Lenoble** of Leicester Square, in the county of Middlesex, for an invention of "certain bituminous mastics or cements capable of receiving various colours, which compositions are applicable to various useful purposes."—17th April 1838.

13. To **Michael Wheelwright Ivison**, silk-spinner, residing in Hailes' Street, Edinburgh, for an invention of "an improved method for consuming smoke in furnaces and other places where fire is used, and for economising fuel, and also for supplying air, heated or cold, to blasting or smelting furnaces."—19th April 1838.

14. To **William Chubb** of Portsea, in the borough of Portsmouth, in the county of Southampton, umbrella manufacturer, for an invention of "certain improvements in night commode pans and chamber-pots."—25th April 1838.

15. To **William Holme Heginbotham** of Stockport, in the county of Chester, gentleman, for an invention of "certain improvements in the construction of gas retorts."—26th April 1838.

16. To **Pierre Armand Lecomte de Fontainemoreau** of Charles's Street, City Road, in the county of Middlesex, for an invention of "an improved method of preventing the oxydation of metals," being a communication from a foreigner residing abroad."—7th May 1838.

17. To **Thomas Ridgway Bridson** of Great Bolton, in the county of Lancaster, bleacher, for an invention of "certain improvements in the construction and arrangement of machinery, or apparatus for stretching, mangling, drying, and finishing woven goods or fabrics, and part or parts of which improvements are applicable to other useful purposes."—11th May 1838.

18. To **John Whyte** of Haddington, in the county of Haddington, ironmonger, for an invention of "certain improvements on stoves for producing heated air, applicable to ovens, or where heated air is required."—11th May 1838.

19. To **Hippolyte Francois, Marquis de Bouffet Montauban**, Colonel of Cavalry, now residing in Sloane Street, Chelsea, in the county of Middlesex, and **John Carvalho de Medeiros** of Old London Street, in the city of London, merchant, in consequence of a communication from a foreigner residing abroad, for an invention of "certain improvements in the means of producing gas for illumination, and also in the construction of burners for consuming gas."—14th May 1838.

20. To **William Neale Clay** of West Bromwich, in the county of Stafford, manufacturing chemist, for an invention of "improvements in the manufacture of iron."—23d May 1838.

21. To **Charles Hullmandel** of Great Marlborough Street, in the parish of Saint James's, Westminster, in the county of Middlesex, lithographic printer, for an invention of "a new mode of preparing certain surfaces for being corroded with acids, in order to produce patterns and designs for the purpose of certain kinds of printing and transparencies."—23d May 1838.

22. To **Jeremiah Grime** of Bury, in the county of Lancaster, engraver, for an invention of "certain improvements in manufacturing wheels which are applicable to locomotive engines, tenders, and carriages, and to running wheels for other useful purposes, and also in the apparatus for constructing the same."—23d May 1838.

23. To **John Upton** of Battersea, in the county of Surrey, engineer, for an invention of "an improved method or methods of generating steam power, and applying the same to ploughing, harrowing, and other agricultural purposes, which method or methods is or are also applicable to other purposes to which the power of steam is or may be applied."—23d May 1838.

24. To **James Hill** of Haley Bridge, in the county of Chester, cotton-spinner, for an invention of "a certain apparatus applicable to machinery used in the preparation of cotton and other fibrous materials for the purpose of spinning."—29th May 1838.

25. To **Edmund Shaw**, of Fenchurch Street, in the city of London, stationer, in consequence of a communication made to him by a certain foreigner,

residing abroad, for an invention of "improvements in the manufacture of paper and paper boards."—29th May 1838.

26. To Alexander Happey, of Basing Lane, in the city of London, gentleman, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "a new and improved method of extracting tar and bitumen from all matters which contain these substances, or either of them."—29th May 1838.

27. To William Ketland Izon, of Cambridge, for an invention of "improvements applicable to steam-engines."—29th May 1838.

28. To John Wilson, the younger of Hurlett, in the county of Renfrew, North Britain, coal-master, for an invention of "an improved process of manufacturing Prussian blue, prussiate of potash, and prussiate of soda, and other substances, into which prussine or cyanogen enters as a constituent."—1st June 1838.

29. To Thomas Hancock, of Goswell Mews, in the county of Middlesex, patent water-proof cloth manufacturer, for an invention of "improvements in the method of manufacturing or preparing caoutchouc, either alone or in combination with other substances."—5th June 1838.

30. To Francis Sleddon of Preston, in the county of Lancaster, machine maker, for an invention of "certain improvements in the machinery or apparatus for spinning and doubling cotton, silk, flax, wool, and other fibrous substances."—5th June 1838.

31. To Robert Thomas, of 36 St James's Street, in the city of Westminster, and county of Middlesex, bootmaker, for an invention of "certain improvements in apparatus to be attached to carriages for the purpose of preventing horses from starting, and for stopping or restraining them when running away or descending hills."—5th June 1838.

32. To Charles Button, of Holborn Bars, chemist, and Harris Grey Dyar, of Mortimer Street, Cavendish Square, gentleman, both in the county of Middlesex, for an invention of "improvements in the manufacture of white-lead."—7th June 1838.

33. To John Potter, of Ancoats, Manchester, spinner, for an invention of "an improvement or improvements in the process of preparing certain descriptions of warps for the loom."—7th June 1838.

34. To William Neale Clay, of West Bromwich, in the county of Stafford, manufacturing chemist, and Joseph Denham Smith, of Saint Thomas's Hospital, in the borough of Southwark, student in chemistry, for an invention of "certain improvements in the manufacture of glass."—7th June 1838.

35. To Samuel Clegg, of Sidmouth Street, Gray's Inn, in the county of Middlesex, engineer, for an invention of "improvements in gas meters."—7th June 1838.

36. To John Melville, of Upper Harley Street, in the county of Middlesex, Esq., for an invention of "improvements in the generation of steam, and in propelling vessels by steam or other power."—11th June 1838.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

On the Life and Writings of the late Professor Rudolphi. By his successor, Professor MÜLLER of Berlin.[*]

THE individual regarding whom I have the honour this day of addressing the Academy, was one of that class now rarely met with in the history of the natural sciences, who combine a solid and productive cultivation of a variety of branches with a rare erudition in these departments. Had he been called away from the career of his development in the very bloom of his strength, it would have been difficult for us to say, if he was greater in the external natural history of organic bodies, or in their internal natural history, that is their anatomy; and whether he had produced more valuable contributions to the anatomy of plants or of animals. When the advancement of the sciences rendered limitation necessary, and when a most laborious appointment required the constant employment of his activity in the natural history and anatomy of animal bodies, this original diversity of his acquirements still animated his later labours, and imparted to them a freshness, whose absence we have too often to regret in the writings of anatomists.

CARL ASMUND RUDOLPHI, Royal Medical Privy-Councillor, Professor of Anatomy and Physiology in the Friedrich-Wilhelms University, and in the Medico-Chirurgical Military Academy, Director of the Anatomical Museum and the Anatomical Theatre, Member of the Scientific Deputation for Medical Affairs, Member of the Academies of Sciences of Berlin, Stockholm, Petersburg, and Naples, Knight of the third class of the Order of the Red Eagle, and of the Swedish Order of the North

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[*] Read at the public meeting of the Berlin Academy of Sciences, August 6. 1835, and published in the Berlin Transactions 1837.

Star, was born on the 14th July 1771, at Stockholm, where his father, who was a native of the territory of Magdeburg, was co-rector of the German School. As to the circumstances of his early life, he has himself left behind him a notice, which Professor Link has published in the Journal of the Russian Society of Medicine, 1833, No. 4, and to which that philosopher has added what was omitted by Rudolphi himself. I have made much use of both these sources of information. Rudolphi received his early education at the German school of Stockholm, and at the Stralsund gymnasium: from 1790 to 1794 he studied at the University of Greifswald, where he dedicated much time to botany. Of his teachers there, he always spoke with the greatest respect. He obtained his degree of Doctor of Philosophy at the same seminary in 1793, after having (prophetically for his future career) defended his dissertation "*Observationes circa vermes intestinales.*" In the year 1794 he attended the lectures of Hufeland and Batsch at Jena, and during the spring of the following year he made a botanical tour by Dresden, Karlsbad, Erlangen, Fulda, Göttingen, the Hartz, to Greifswald, where, after defending the second part of the same dissertation, he obtained his degree of Doctor of Medicine. Since 1793 he had been private lecturer in the Philosophical Faculty of Greifswald, and in 1796 he became private lecturer in the Medical Faculty. In the winter of the same year he went to Berlin, in order to have practice in dissecting; and in the course of the following year he became *adjunct* in the Medical Faculty and *prosector*. In autumn 1801 he again proceeded to Berlin, to instruct himself in veterinary surgery, on which subject the professorship at the veterinary institution at Greifswald was transferred to him. He was occupied there till the year 1810, after being named ordinary professor of medicine in 1808. To this period of his life belong some of his most important writings.

In the year 1802 Rudolphi published his anatomical-physiological treatises. He wrote this work after the death of his first wife, in a very agitated frame of mind. "When I was writing that book, I was often obliged to spring up and give free course to my tears, over the loss of her who had at so early a period preceded me:" so writes this noble-minded man at the beginning of a manuscript remark written long afterwards in his own copy of the work. Besides some new essays, Rudolphi also includes in this publication remarks on subjects previously investigated by him. He first of all treats of different portions of the eye, proving that the *zonula* is a different structure from the *retina*; of the crossing of the nerves of sight in fishes; of the structure of the teeth; of the ventricles of the brain; of hydatids; of the breathing of frogs; of the structure of the *villi intestinorum*, and of Peyer's glands. In the last essay he describes, in reference to many different animals, the varieties of external structure of the glands of Peyer, bodies which, not long previously, were represented as accidental and diseased occurrences. How correct he was, has been proved by the importance which this subject has subsequently acquired. Rudolphi, in discussing these puzzling bodies, did not treat of their internal structure, which perhaps, at that time, he alone could have explained. This investigation was not rendered so necessary till the *cholera* and the *typhus abdominalis* had surprised many in their ignorance of the existence of those structures, which Rudolphi had as it were introduced anew into the science.

By his essay on the *villi intestinorum*, Rudolphi obtained a still more important position among the anatomists who had occupied themselves with the structure of the tissues; and although he denied the existence of vessels in the *villi*, and refused to admit the presence of *villi* in fishes, yet his observations have shewn the errors of the ancients in respect to the visible openings of these portions: he also distinctly pointed out in some cases the *epithelium* of the *villi intestinorum*, and by examining the variations in the occurrence of these organs, placed limits to the physiological hypotheses. Rudolphi remained almost quite general in the views taken in this work. In his own copy of the book, there is the following manuscript note: "*Librum duodecim annis elapsis legi anatomicus duodecies melior ac tum temporis eram, plurima tamen probo.*"

In the year 1802, Rudolphi made a journey through a portion of Germany, Holland, and France; and published (1804) his observations in the departments of natural history, medicine, and the veterinary art. This tour developed a wonderful abundance of knowledge in botany, zoology, pathological anatomy, and veterinary medicine, and is rendered extremely valuable by the many interesting observations emanating from the meeting of so richly informed an individual with the first scientific men of Germany, Holland, and France. Of all his writings, these contributions to anthropology, general natural history, and physiology, are the most calculated to convey a just impression of his character to those who were not personally acquainted with Rudolphi. Who could help admiring that man, whose sound judgment was so conspicuous,—whose unfettered, open, and straight-forward mind was wholly devoted to the investigation of realities,—and who always guarded against that unproductive and fanciful tendency which he sometimes encountered. How mild and yet how correct are his opinions; how interesting are his remarks on the medical men and institutions of Berlin at that period; and how attractive are the accounts of his meetings with Brugmans, Cuvier, Tenon, Richard, Gall, and lastly the extraordinary Beireis, whose delineation as given by our author was not less interesting than that given by Goethe. Rudolphi's work contains such extended notices of what he had seen, that it is still a valuable assistance in making use of foreign establishments.

That Rudolphi's knowledge of botany was varied and extensive, is proved by his scattered observations made during this journey, by his various essays, and especially by his Anatomy of Plants, published in 1807. On this subject it is better that my colleague Professor Link should speak than myself, and he has kindly communicated the following notice of Rudolphi's botanical studies.

Rudolphi was attracted to the subject of botany by Weigel of Greifswald, and he combined that science with the study of anatomy in the same manner as Haller, whose example had also in other respects great influence over him. He collected with diligence, observed in the garden at Greifswald, obtained by mercantile connection plants from Barcelona, and received plants from Lisbon from a friend. The *Ornithogalum Rudolphii* of the Greifswald garden still retains his name. His descriptions of plants appeared in Schrader's journal. Willdenow named after him a genus of plants belonging to the natural order Leguminosæ, and we still possess it in our garden. Sprengel directed his attention to the anatomy of plants, and he competed for the botanical prize offered on this subject by the Göttingen Society. It was divided between him and his friend Link. They had often previously written on the subject to each other. The examination of the cellular tissue is the weakest part of his essay; but he declares himself decidedly, and with his usual keenness, an opponent to the system of Mirbel, who founded the whole physiology of plants on the pores he alleged to exist in the walls of the cells and vessels. The description of the tracheæ is more exact, and he regards them as vessels for nourishment. But his investigations respecting the slits, pores, and *stomatia*, on the green portions, as to which he had examined a great many plants, are still truly classical, and are the groundwork of our knowledge of the subject. Similar to these is his investigation of the air-vessels of plants, which has not been equalled by any subsequent attempt. Lastly, he again examined those singular radiated bodies in the cells of the Nymphaeæ, and other water plants, and made the best observations on them that have hitherto been published.

In the mean time, the call which Rudolphi received in 1810 to a sphere of much greater activity, removed him for ever from his botanical pursuits. As professor of anatomy and physiology, director of the anatomical establishments, member of the scientific deputation for medical affairs, and of the Academy of Sciences, he now laboured during twenty-two years for anatomy and physiology with splendid success.

Walter could not be surpassed as a practical anatomist, and by his writings he holds a rank as one of the first anatomists; but microscopic anatomy, in which Rudolphi had previously distinguished himself, was unknown to him: he had accomplished so much with the naked eye, that he considered that anatomy had been nearly perfected, and still how much is there yet to discover with the naked eye. Walter had not prosecuted comparative anatomy, and it was therefore necessary for his successor to furnish materials to the University. Before the time of Rudolphi there existed nothing in comparative anatomy, except the preparations in the veterinary school, and a few objects which were private property. When Walter's collection was purchased by the King in 1803, it contained 3071 preparations, which were chiefly of human anatomy. From that time till 1810, the collection received, under Walter's superintendence, an addition of 162 preparations; under Rudolphi's direction it was increased by the accession of 3964 preparations. At the same time the magazine received several thousand objects, of which the larger portion were already prepared, but could not be displayed on account of the expense.*

Rudolphi had therefore the merit of founding the zootomical museum; and although this collection cannot compete with the museums of Paris and Leyden in regard to skeletons, yet it can bear comparison with the first foreign collections as to preparations of the soft parts; in the department of human and pathological anatomy our collection holds a distinguished place even among the best. It must also be remembered, that the museum has only existed since 1803 as a public establishment,—that the collection was extended so as to include comparative anatomy only since the founding of the university in 1810,—that our zootomical means are more limited than those of London, Paris, and Leyden,—and that our commercial connections are inconsiderable. A maritime nation is, in this respect, in possession of extraordinary advantages; and our wonder is with justice excited in hearing that the Garden of Plants at Paris, with its mineralogical, botanical, zoological, and anatomical collections, supports eight travellers in the most remote quarters of the globe.†

Rudolphi, by his talents for teaching, and by his personal qualities, soon acquired a high reputation; and not only on account of his writings, but also as a professor, he was regarded as one of the brightest ornaments of the University. That he communicated a powerful impulse to the study of comparative anatomy, is proved by the many excellent inaugural dissertations, which partly describe the remarkable preparations in the anatomical museum, and partly originated from them. A feeling has often been remarked in the greatest men of unwillingness to communicate to others their own methods, and to cultivate talents which might interfere with themselves. In this respect Rudolphi had great merit, for he communicated not only his instructions but his enthusiasm to his pupils. He was easy of access to the young; and although those with recommendations might receive no particular assistance from him, yet every one who exhibited good qualities, received every advantage without any introduction whatever. Students, and our own as well as foreign medical men and naturalists, found themselves at home in his library; and as he urged forward the young by his instructions, animated them by his advice, and, with the liberality of a Banks, aided them by means of his library, of the anatomical museum, and the objects which he himself had collected, zealous students were not awanting who devoted themselves to anatomy under his particular superintendence. His enthusiasm for the science, his love of truth, his noble and disinterested character, his violent opposition to false directions of investigation, carried his students forward with a power that was irresistible. Such qualities in a teacher make an indelible impression on the youthful spirit, and communicate an impulse which ceases only with life itself; and I shall never forget the impression I received from Rudolphi: it was he who laid the foundation of my love for anatomy, and he who decided the direction of my pursuits. I enjoyed for a year and a half his instructions, his counsel, his paternal friendship; and when I left him, he presented me with many means of prosecuting my scientific studies; his kind interest accompanied me afterwards when our views often widely differed, and when he saw unwillingly that I occupied myself with the more abstract subject of the physiology of the mind, rather than with the investigation of the anatomy of the organs of the senses, such as that of the eyes of insects and spiders. With his prosector and his colleagues Rudolphi was on the most friendly terms. Every one must have remarked his respectful conduct towards Knape, the colleague next him in the department of anatomy. In the faculty and in the senate Rudolphi was distinguished in a remarkable degree for his participation in business, and for the correctness and decision of his strongly expressed opinions; and his valuable labours rendered important and influential his situation in the scientific deputation of medical affairs.

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* By the exhibition of a great many preparations which had been prepared by Rudolphi, but which could not be displayed in his time, and of preparations made since that period, the number of the objects in the anatomical museum now amounts to 11,000, in which only the perfect ones are reckoned, and the abundant materials of the magazine (about 3000) are not included. The preparations of the comparative anatomy of the races of the human species are 214 in number, among which there are 16 skeletons of non-European races, and 134 skulls of different races. The osteological collection of vertebrate animals, includes 434 entire skeletons of mammalia, 336 of birds, 154 of amphibia, and 279 of fishes. The pathological department of the anatomical collection is particularly rich in monsters, in diseases of the bones, and specifically determined tumours.

† *Rapport sur les besoins du museum d'histoire naturelle pour l'année* 1835, *presenté au ministre de l'instruction publique, Paris* 1834.

In the year 1812, Rudolphi published his contributions to anthropology and general natural history. The biography there given of Pallas, and the essays on the arrangement of animals according to the nervous system,—on the distribution of organic bodies,—and on the relations of beauty in the two sexes, are among the most attractive of his writings. In the arrangement of animals, Rudolphi proceeded from the anatomical-physiological principle, and from that system of organs which has most to do with imparting form to all the others, viz. from the nervous system. The vertebrate animals with the spinal system he termed *Notoneura*, also *Diploneura*, on account of the simultaneous occurrence of the spinal nervous system, and the *Nervus sympathicus.* He named a second division *Gastroneura* or *Myeloneura;* in these the nervous cord lies in the stomach. In the third system he includes the animals with scattered gangliæ; and in the fourth those whose nervous system is still unknown, which he terms *Cryptoneura.* Although many invertebrate animals possess the system of nerves of motion and sensation, and that of the organic nerves, and although both systems of nerves may extend through the whole animal world, yet the principle of arrangement is nevertheless striking, and exhibits to us in clearly marked differences the chief subdivisions of animals, although not of those that are the lowest in the scale.

Rudolphi acquired the greatest reputation by his labours on the natural history of intestinal worms. If the history of the natural sciences were to dwell only on the names of discoverers, those who had ascertained important facts from which many others were explained,—who have disclosed a multitude of forms, and the structure of entire classes of natural bodies,—

and who have discovered and applied the principles for the arrangement of these, then Rudolphi's name would be rendered imperishable by his investigations regarding the Entozoæ alone. Linnæus had indicated only eleven species of intestinal worms in the 12th edition of his *Syst. Nat.;* Gmelin, in the 13th edition, 299; Zeder, 391. Rudolphi's first great work on intestinal worms, *Entozoorum historia naturalis*, which appeared in three volumes, between 1808 and 1810, before his removal to Berlin, contains the description of 603, principally accurately described species. He almost doubled that number by his own personal observations, made more especially during a journey to Italy, which he undertook in 1817, chiefly for the prosecution of this subject; by the communications of his intimate friend Bremser; and by the contributions sent from Brazil by Von Olfers and Natterer. His *Entozoorum Synopsis*, published in 1819, contains descriptions of 552 accurately described, and 441 doubtful, altogether 993 species.

Rudolphi made known many valuable observations on the anatomy of intestinal worms; and what he says in favour of *generatio æquivoca*, is still almost the only recorded expression of opinion on which the defence of this doctrine can be made to rest. Rudolphi's classification is still regarded as the most approved one. Anatomy has here, it is true, made great progress under the guidance of Mehlis, but it has not authorized us to arrange these so widely differing animals in already existing divisions of the remaining ones; and hence, in the present state of the science, the best plan would be simply to place next one another the natural groups of the worms of fresh and salt water, and of internal worms, so that the Annulata, the Turbellaria of Ehrenberg, the Nematoidea of Rudolphi, the Trematoda of the same, and the Tænia, should stand next one another, whether one of these anatomically different groups should live within or without the animal body. One point in which I cannot agree with Rudolphi is his separation of the Cestoidea from the Cystica (Blasenwürmer). It seems rendered inadmissible by a more close examination of the Tetrahyncha and the Anthocephala, which are similar to them; these last were removed by Rudolphi to the *Cystica*. This division of the Cystica contains animals which are not more like one another than Cystica are like the Cestoidea. The long tape-worm-shaped species of the *Cysticercus* (*C. fasciolaris*), form the passage from the Cestoidea to the other Cystica. The heads of the *Cœnurus* and *Echinococcus* are tape-shaped; and the Tetrahyncha, placed under the Cestoidea, have, apart from the consideration of the proboscis, a considerable resemblance to the *Echinococcus*, although they do not live in vesicles, and degenerate in vesicles. Hence Wiegmann has already remarked, that the animals of the Cystica tribe are repetitions of the Botriocephali (Grubenköpfe) and Tæniæ (Bandwürmer), and are to be regarded as undeveloped forms of the same. According to my opinion, the *Cestoidea* and the *Cystica* must be included in the same order, and form two different sections. What is known of the development of the Tæniæ is favourable to this arrangement; for, according to Mehlis, several Tæniæ, at an early stage, consist only of a head portion. This apparent connection, which proceeds from the arrangement of the members, may be strongly or faintly expressed in the two sections. In the Tæniæ, it has relation to the multiplication of members and the sexual portions. Some animals of the Cystica section, on the contrary, as the *Cœnurus* and *Echinococcus*, appear as really compound animals, with a common origin ("*Blase*" or vesicle), and many heads.

If we can now explain more readily these connecting circumstances, we owe it to Rudolphi. He introduced order and method into this new Fauna of nature,—inasmuch as he investigated and defined, in all its relations, an almost new region of natural history. It is seldom that Germans, in their home undertakings, have had the good fortune to investigate the natural bodies of foreign countries. Forster, Pallas, Lichtenstein, Tilesius, and Kuhl, went to a distance, when they attached themselves to undertakings connected with foreign countries. The limitation caused by our geographical position, has, on the other hand, imparted to our spirit a certain direction towards what is concealed, and has made us so much the greater in the investigation of a world of concealed inhabitants of our native creatures, viz. the Entozoa,—in the investigation of the structure of natural bodies, and of their internal living processes.

In his natural-historical writings, Rudolphi united the method of Linnæus with that of Pallas. His diagnoses are simple, short, and distinct, like those of the great Swede; in his extended descriptions he always has especial regard to anatomy. In his researches on intestinal worms,—on the *Balæna rostrata* and *longimana*,—on the *Rana pipa*, and other osteological monographs;—in his writings on electrical fishes, whose nerves and organs he understood better than any of his predecessors,—in his essays on the orang outang, and on the embryo of apes, we remark this union of natural history, and anatomy: characteristic description of nature appears also again in his physiology; and what he says of the races of man, and of the mental qualities of the two sexes, may be regarded as a model of the natural-historical treatment of these subjects.

The Transactions of the Academy of Sciences, contain a series of valuable treatises by Rudolphi. Those on comparative anatomy are partly osteological, like some of those already mentioned; partly neurological, such as the essays on the electric eel and silurus; and his observations on the sympathetic nerves, in which he describes the portion of the *sympathicus* only indicated by Sömmering as running along with the *arteria vertebralis;* and partly myological, as the memoir on the anatomy of the lion. Among his labours in pathological anatomy, I would quote especially those on hydrocephalus, and on a human monster consisting of a mere head. In the last case, of which I obtained two years ago a counterpart, he shewed, first of all, how similar productions without a heart, which have given rise to so many hypotheses, can be nourished,—inasmuch as that head was united with the umbilical cord of a second perfect fœtus, and its vessels were branches of the vessels of the umbilical cord; which was also the case in the instance that came under my own observation. His treatise on the hydrocephalus of the embryo, seems to me to be of still greater importance; for it explains a multitude of innate defects in the development of the brain and skull, as proceeding from the same source. I am surprised that Rudolphi, who brought many facts of pathological anatomy under the same law, did not apply the idea of a secondary destruction or interruption of the development also to the explanation of other defects, as he so interpreted them, that the germ was only sufficient for the production of a head, a foot, &c. An embryo, rachitic at the earliest period, and in which the head is as large as the rest of the body, is not far removed from the imperfect development of the whole lower half, and from the insertion of the umbilical cord under the head. The paper on hermaphrodism is equally distinguished by learning and acuteness. Rudolphi considers this phenomenon in its most general point of view, and examines it in most classes of animals. The usual formations termed hermaphroditic, which are nothing else but obstructed formations of the male genital organs, or progressive metamorphoses of the female parts, are properly excluded by him; but he describes a rare real human hermaphroditic case, in which, on the one hand, the testicles and *ductus deferens*, and, on the other, the *uterus* and *tuba*, were present. This instance is a very remarkable one, although I cannot convince myself of the existence of an ovary on the female side, which Rudolphi assumed. As anatomists generally do, Rudolphi ascribed great importance to this deviation in the structure of the animal and human body. When one is accustomed to attend to every thing with mental acuteness, and is enthusiastically devoted to his subject, it often happens that, though what is extraordinary may sometimes be overrated, yet the deviation from the rule often leads to the knowledge of the law, which is above the rule. Cuvier, to whom pathological anatomy was unknown, could not acquire any taste for pathologico-anatomical minutiæ; and it is extremely characteristic what Cuvier on one occasion replied to Rudolphi, while the latter was conversing with him in Paris on rare pathologico-anatomical curiosities: " *Mais ce n'est qu' accidentel.*" Rudolphi relates this in the account of his journey. It must, however, be confessed, that Cuvier's countrymen, apart from the theory of innate monstrosities, in which the Germans have done so much, have known how to make the most of the cultivation of accidental phenomena for the subject of medicine. This connecting of practical medicine with anatomy, must arise in a country where Bichat appeared and developed the laws of the sound and diseased textures.

But Rudolphi was equally zealous for all branches of anatomy. He often expressed the opinion that an individual could not be sufficiently informed in one branch, and could not contribute anything of consequence, without being perfectly acquainted with all the other branches. Solid knowledge in zoology is also necessary for the productive cultivation of comparative anatomy. Hence he desired that anatomists should comprehend in their studies human, comparative, and pathological anatomy, although they might not prosecute all these departments with equal fondness; and he sometimes blamed severely the errors committed by anatomists from imperfections in their studies, from one-sided knowledge, or actual want of information.

Rudolphi was an opponent of the kind of "*Naturphilosophie*" which prevailed for a season. On every occasion he declared his hostility to a species of natural studies combined with a misunderstood philosophy which expressed itself for some time with considerable pretensions, owing to the want of an exact method, and to a strong tendency to generalization. The powerful observations made by him, in his biography of Pallas, as a warning to students, cannot have been without the desired effect; and the sentiments are just as remarkable which he has inserted in the article Anatomy, written for the "*Encyclopädisches Wörterbuch der medicinischen Wissenschaften.*" It is not to be doubted that he there recognises the influence of comparative anatomy on the right understanding of the laws of formation. In that essay, as well as in his lectures, he spoke in favour of the existence of a pair of vertebræ in the skull, and only blamed the abuse of this idea, which, I may take this opportunity of mentioning, neither Goethe, nor Oken, nor Dumeril first expressed or published, but J. P. Franks who was so fortunate as to throw it out in his work *De curandis hominum morbis*, 1792, *lib.* 11. *p.* 42. If Rudolphi in his labours entered but little on such questions, it might arise from this chiefly, that the arbitrary manner in which the "*Naturphilosophie*" had treated these subjects had rendered the whole matter disagreeable to him. It has sometimes appeared to me as if Rudolphi had, in anatomy, placed too little value on this knowledge of the laws of formation. The discovery that all embryos at an early period have gill-arches on the neck, did not at all suit his ideas; he suspected deception, and appealed to other explanations. For a long time he carried on experiments on incubation; the results were not exactly in favour of the idea of the gill-arches; but still there was much observed in the course of these experiments, which shewed a far greater resemblance of the state of the fœtus in birds to that of fishes than might have been anticipated. The idea that man during his development, passed through the other degrees of animal life, was quite in opposition to his views, and in that he was quite right. The existence of gill-arches in the neck of embryos, would not, however, have disturbed Rudolphi, had he not perhaps also rejected the idea of unity of organization in the different classes of vertebrate animals, along with the idea of the passing through the various grades of animals. How he explained these undoubtedly existing arches and slits in the neck of embryos, has never been very clear to me. He probably already adopted the proper opinion, that the general plan for all vertebrate animals is at first similar; that, however, it is only in fishes that gills are formed on those arches, while in other animals these arches partly gradually disappear, and partly become changed into the horns of the *os hyoides*. In his doubts respecting opinions which were adopted by others, but not by himself, Rudolphi was neither reserved nor obstinate: good arguments always prevailed with him, and he willingly gave up his opinion when convinced of its want of foundation. He had not seen the connection of the *vesicula umbilicalis* with the intestinal canal by means of a passage, probably because he examined old eggs; still in 1828 he was prejudiced against it. Professor Gurlt shewed him the connection of a *diverticulum ilei* with the navel, and he became doubtful respecting the explanation he had given.

Rudolphi's tendency in physiology was the criticism of observations and of prevailing doctrines. The time at which he began his labours was a brilliant one for physiology. After the discovery of galvanism, by Aloysius Galvani, that phenomenon was for a long time regarded by the first natural philosophers and physiologists as a physiological one. When this view was afterwards disproved there was still an opportunity of discovering the laws of animal irritability, and on this path, which was opened by A. von Humboldt, many natural philosophers and physiologists followed. Rudolphi so far took part in this investigation as to examine the hypothesis of sensible atmospheres of the nerves; and the grounds on which he controverted the proofs deduced from galvanic experiments on animals, hold still good at the present day. After it was perceived that galvanism is only a stimulant for the powers of the parts of the animal body, and after the application of this stimulant on the animal fibres had afforded the physiologists all that could then be obtained, it was discovered that too much had been expected for the science of physiology from that discovery, and that medical men, instead of employing this means, under new and productive points of view, for further investigations, became alienated from it. What was now easier for many than to give themselves up to the deceptions of a physiological mysticism or magic, which always made the so called animal-magnetic power more full of pretension and more infectious, and which interpreted the difficulties of physiology in a more convenient and simple manner. How melancholy is the picture of those strivings,—how depressing in contrast to the hopes of that period in which the work on the excited fibres of the nerves and muscles made its appearance, and shewed the method in which advances were to be made. A prevalent arrogant and often superficial manner of philosophizing on natural objects could offer to those who really reflected in the face of this giddiness, but little that was consolatory. Even in Berlin, the focus of the most esteemed scientific exertions, credulous individuals were not awanting. There it was that Rudolphi checked the diffusion of such opinions by his vigorous opposition, and great gratitude is due to him for turning back medical men from the field of medical superstition. Other examples might be adduced of the services rendered by Rudolphi's candid expression of opinion against false directions of investigation. We still enjoy the beneficial results—they are similar to those of the operation of the yearly reports by the great Swedish chemist on the more exact prosecution of the sciences.

Rudolphi gave an abstract of his physiological doctrines in his Elements of Physiology (*Grundriss der Physiologie*), of which the first volume appeared in 1821, the 1st part of the second volume in 1823, and the 2d part in 1828. The last part is awanting; it was to have treated of the excretions, and of generation. Among his papers there was found only a fragment on the secretion of urine. The work seemed latterly to have lost interest for him, especially as this department of physiology had in other quarters made great progress, and Rudolphi preferred treating of those subjects in which he could make use of his own investigations. Good criticism of observations, a wonderful learning, and the employment of a rich treasure of valuable anatomical observations, characterize this excellent work. Compared with other treatises, in regard to the doctrinal department, much is certainly wanting to which we are generally accustomed, and some things even which belonged to the actual state of the science; on many points he was short, when he had no critical remarks to make, and no personal observations to quote; and he had not bestowed the necessary degree of attention on the progress of the department of the nerves. Finally, the unusual abundance of facts in comparative anatomy, and the criticism of many details, in which Rudolphi, on account of his investigations, was more copious, in some measure concealed the actual deficiencies and imperfections of our science. This excellent work will always possess great value, when many writings, which contain more physiological facts, but more error, shall have been long forgotten.

Rudolphi's predominating inclination in physiology was anatomical and sceptical; his physiological writings were chiefly important in refuting prevailing opinions. He did not regard physiological experiments at all in the same light with the certainty of anatomy. There is no wonder that this excellent man, who on all occasions expressed his aversion to *vivisection*, assumed a hostile attitude against all hypotheses and ill-grounded physiological experiments. We must participate entirely in his just indignation in perceiving how many physiologists who shewed their eagerness to render physiology a science of experiment, by an ill-directed system of opening and tormenting multitudes of animals, from which results were obtained, often of a very insignificant and unsatisfactory nature. But Rudolphi went too far when he believed that experiments on animals teach us nothing. Experiments instituted on important questions, have here, as well as in physics, led to the greatest discoveries. The discovery of the different functions of the anterior and posterior roots of the nerves of the spinal cord, was originally certainly the idea of a mind of genius, which it was necessary should be confirmed by the experiments of him and of others. Rudolphi, however, did not remain altogether indifferent to the modern development of the physiology of the nerves. At his suggestion, and under his own eye, many experiments were made in 1823, in the veterinary school, with the view of examining Bell's views regarding the *nervus facialis* and *trigeminus;* and although at first he was doubtful of the different functions of the roots of the nerves of the spinal marrow, probably because he did not confide with any certainty in the existing physiology of such questions of vital action, yet afterwards, when decidedly confirmatory experiments were made known, he declared openly in favour of the matter, and considered it as one of the greatest advances in physiology. Rudolphi avoided a more philosophical division of the general relations of vital activity, which was to him less safe than the criticism of facts, and he entered on the province of the mind with caution, and chiefly only in such a manner as that he speedily passed to a natural historical view of the facts, where he was always successful. Among the more general physiological writings, he marked out but few in which he perceived consistent reasoning and acuteness; and though he acknowledged the imperfections of Reil's treatise on vital power, and his derivation of all the phenomena of life from mixture and form, yet he nevertheless regarded that work as a masterpiece, and always spoke of Reil with the highest respect.

Rudolphi was deeply grieved with the relation in which he stood to Meckel. Each acknowledged fully the merits of the other, and yet they could not get the better of mutual taunts; and these, although they were regarded by no one but themselves, embittered the life of each. Rudolphi's straightforward, though never severe, mode of expressing his opinion in his writings produced much vexation to himself, and this would not have been unexpected by him, had he known mankind more accurately, and had he not adopted the mode of thinking of any other but himself. Professor Link has finely remarked, that our author was too guileless to make mankind the subject of his observation; and I would add, that nothing was more painful to him than to discover that he had been deceived in individuals.

Rudolphi's early sound health had for some years been perceptibly deteriorated; formerly the dissecting-room was always too warm for him, he then required so much coolness about him, that the others suffered; in later years, he could not have the apartment warm enough. When I saw him in 1828, the first time for five years, I was distressed to see how much sharper and more serious his features had become; he looked much older, although his acute power of sight still rendered him quite equal to all minute investigations, where certainty of the hand was not requisite. I had rejoiced at the opportunity of seeing again my paternal friend, and I then saw him for the last time. This was apparent to me, and the feeling was a mournful one; the change was but too striking, from the former happy and bright expression of his features. Rudolphi retained his full activity, however, till the last year of his life; in August 1832, *ascites*, caused by an affection of the liver, began to establish itself, and of this complaint he died on the 29th November of the same year. His collections were purchased by the king; and his Entozoæ are now in the Zoological Museum; while his private library forms part of the Royal Library, and his collection of medals is incorporated with the Museum of the Fine Arts.

Rudolphi, considered as a private individual, was not less estimable than in his character as a scientific man; *integer vitæ scelerisque purus.* Whoever saw him, loved and respected him; and though his candour sometimes offended, yet, in the long run, the irritation thus produced was constantly overcome. The chief qualities he sought for in his fellow-men were uprightness, sincerity of intention, and the freedom of the mind from every ignoble disposition. Where he found these virtues, he gave up every thing else, and did not allow himself to be deceived by the semblance. He expresses his genuine sentiments in his poems, in which he so often treats of friendship. I cannot help feeling deeply moved whenever I think on the open, serene, and impressive features of his countenance, and the amiable and manly earnestness combined with the energy and candour of his character. When the tone of my mind is lowered, I would shun the sight of the image of my paternal friend; and when I wish to recall the most elevated events of my life, I immediately recur to Rudolphi.

What my lamented friend once declared to be his highest wish has been granted to him. Even as a boy he loved Linnæus; in his verses he sings his praises with enthusiasm. It is he who appears to him, and leads him into the temple where the tablets are inscribed with the names Hedwig, Gärtner, Thunberg. At that time he did not anticipate how near he stood to the monument of Hunter, Daubenton, and Vicq D'Azyr. One tablet was empty, and over it was impenetrable darkness; it is now provided with an inscription. There shine also Pallas and Peter Camper, and that individual, who, while he yet lived, wished for a place at the feet of Peter Camper, Bojanus.*

CATALOGUE OF RUDOLPHI'S WRITINGS.

A. BOTANICAL WRITINGS.—(1.) Botanical Observations. In Schrader s *Journal für die Botanik*, 1799. Vol. II., Part IV. Göttingen. 1799. 8vo, I. No. 4, p. 274. (2.) Botanical Remarks. In the same, 1800, Vol. II. Parts I. and II. Göttingen, 1801–8, I. No. 8, p. 201. (3.) On the Anatomy of Plants. Prize Essay written for the Göttingen Society of Sciences. Berlin, 1807, 8vo, with six Plates.

B. ZOOLOGICAL WRITINGS.—(4.) *Observationes circa vermes intestinales, Gryphiswaldiae*, 1793, 4to. (5.) *Observationum circa vermes intestinales Pars II. Gryphiswaldiae*, 1795, 4to. (6.) Observations on intestinal worms. In Wiedemann's *Archiv für Zoologie und Zootomie*, Vol. II. Part I. Brunswick, 1801, 8vo, No. 1, p. 1. (7.) Continuation of same in the same Journal, with one Plate, Vol. II. Part II. 1802, No. 1. p. 1, Plate 1. (8.) Continuation of the same, with one Plate. In the same Journal, Vol. III. Part I. 1802, No. 2, p. 61, and Plate 2. (The conclusion of these observations was promised but never appeared.) (9.) New observations on intestinal worms. In the same Journal, Vol. III. Part II., 1803, No. 1, p. 1. (10.) *Entozoorum sive vermium intestinalium historia naturalis. Amstelaedami*, Vol. I., 1808; Vol. II. Part 1, 1809, Vol. II. Part 2, 1810, 8vo, cum. xii. tab. aen. (11.) First supplement to my natural history of intestinal worms. In: *Der Gesellschaft naturforschender Freunde zu Berlin Magazin für die neuesten Entdeckungen in der ges. Naturkunde.* Jahrg vi. Berlin, 1814, 4to, Quart. 2, 1812, No. xii. p. 83. (12.) *Entozoorum Synopsis, cui accedunt mantissa duplex et indices. Berolini*, 1819, cum 3, tab. aen.

C. MIXED ANATOMICAL AND PHYSIOLOGICAL WRITINGS.—(13.) Anatomy. An article in the "*Encyclopædisches Wörterbuch der Medicinischen Wissenschaften*," edited by Gräfe, Hufeland, Link, Rudolphi, and Siebold, Vol. II. Berlin, p. 357. (13. *b.*) Same article published separately. (14.)

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[*] Vide Notice of Bojanus in Edinburgh New Philosophical Journal, vol. viii. p. 200.

Contributions to Anthropology and general natural history. Berlin, 1812. 8vo, with a likeness of Pallas. (15.) Anatomical-Physiological Essays. Berlin, 1802, with eight Plates.

D. Writings on Comparative Anatomy. (16.) On the Anatomy of the Lion, with five Plates. In the Transactions of the Berlin Academy of Sciences for the years 1818–1819. Berlin, 1820. Physical Class, p. 131. (16. *b.*) Same article published separately. (17.) *Resp. C. Guil. E. Reimann : (diss. Med.) spicilegium observationum anatomicarum de hyaena. Berolini,* 1811, 4to, cum. 1. tab. aen. (18.) Anatomical Remarks. (1. On the Orang-Outang, and proofs that it is the young of the Pongo. 2. On the electric Silurus), with five Plates. In the Transactions of the Physical Class of the Berlin Academy for the year 1824. Berlin, 1826, p. 131. (19.) On the embryo of Apes and some other Mammalia, with four Plates. In the same for the year 1828. Berlin 1831. Phys. Class, p. 35. (20.) Anatomical Observations, (1. On the bones of the back part of the head of the Pelecanus Carbo L. 2. Remarks on the Eye. 3. On a rare species of hermaphrodism in the *Simia capucina L.*) with two Plates. In the same for the years 1816–1817, Berlin, 1819. Phys. Cl. p. 111. (21.) Some Anatomical Remarks on the *Balæna rostrata*, with five Plates. In the same, for the years 1820–1821. Berlin, 1822. Phys. Cl. p. 27. (22.) On the *Balæna longimana*, with five Plates. In the same, for the year 1829. Berlin, 1832, Phys. Cl. p. 133. (23.) Observations on Comparative Anatomy (1. On the Electric Fishes. 2. On the poison spur of the male *Ornithorynchus paradoxus*), with three Plates. In the same for the years 1820–1821. Berlin, 1822, Phys. Cl. p. 223. (24.) Some remarks on the Structure of the Breast, and supplementary observations. In the same, for the year 1831. Berlin, 1832, Phys. Cl. p. 337. (24. *b.*) First paper on the Breast, published separately. (25.) Some remarks on the crossing of the nerves of sight in fishes. In *Wiedemann's Archiv für Zoologie und Zootomie* Vol. I. Part II. Brunswick, 1800. No. 5, p. 156. (26.) *Resp. F. Guil. Breyer : (diss. med.) observationes circa fabricam ranæ pipæ. Berolini,* 1811. *Cum.* ii. *tab. aen.* (27.) *Resp. F. Ch. Massalien : diss. sistens descriptionem oculorum scombri, thynni et sepiæ. Berolini,* 1815. *Cum i. tab. aen.* (28.) *Resp. L. Wolff : diss. anat. de organo vocis Mammalium. Berolini,* 1812. *Cum iv. tab. aen.*

E. Writings on Human Anatomy, and on General Anatomy.—(29.) *Resp. E. M. H. Schwarz : disp. anat. de pilorum structura. Gryphiæ,* 1806. (30.) *Resp. C. F. L. Gantzer : diss. anat. musculorum varietates sistens. Berolini,* 1813. (31.) *Resp. Sels : diss. musculorum varietates sistens. Berolini,* 1815. (32.) Some observations on the *villi intestinorum*. In Reil's *Archiv für Physiologie*. Vol. IV. Halle, 1800, p. 63. (33.) Continuation of the same ; in the same, p. 339. (34.) *Diss. de oculi quibusdam partibus. Gryph.* 1801. (35.) *Resp. J. H. Carger : diss. de ventriculis cerebri. Gryphiæ,* 1796. (36.) Some observations on the sympathetic nerves. In the Transactions of the Berlin Academy for the years 1814–1815. Berlin, 1818. Phys. Cl. p. 161. (37.) *Progr. de solidorum, c. h. partibus similaribus. Gryphiæ,* 1809. (38.) *Resp. J. L. Held : (diss.) observationes circa dentitionem. Gryphiæ,* 1809. (39.) Contribution to the history of the teeth. In Reil's *Archiv für Physiologie*, Vol. III. Halle, 1779, p. 401. (40.) *Resp. J. G. Tesmer : dis. anat. sistens observationes osteologicas. Berolini,* 1812. *Cum* 2. *tab. aen.* (more especially on the teeth.) (41.) On the formation of horn. In the Transactions of the Berlin Academy of Sciences for 1814–1815. Berlin, 1818. Phys. Cl. p. 175.

F. Physiological Writings.—(42.) On the sensible atmospheres of the nerves. In Reil's *Archiv für die Physiologie*, Vol. III. p. 188. (43.) On the sensible atmospheres of the nerves. In the Transactions of the Berlin Academy for 1812–1813. Berlin, 1816. Phys. Cl. p. 208. (44.) *Dubia contra J. Galli de organis in cerebro distinctis iisque Cranii ope detegendis hypothesin. Nova Act. Academiæ Scientiarum imp. Petropolitanæ. T. 14. Petropoli*, 1805. (45.) Elements of Physiology. Berlin, Vol. I. 1821 ; Vol. II. Part 1st, 1823 ; Part 2d, 1828.

G. Pathological-Anatomical Writings.—(46.) General view of the calculi which have hitherto been found in vertebrate animals. In the Transactions of the Berlin Academy for 1812–1813. Berlin, 1816. Phys. Cl. p. 171. (47.) Description of the brain of a child, where the right eye and the nose were awanting, with two Plates. In the same, for the years 1814–1815. Berlin, 1818. Phys. Cl. p. 183. (48.) On a human monster, consisting only of a portion of the head and neck, with four Plates. In the same, for 1816–1817. Berlin, 1819. Phys. Cl. p. 99. (49.) On hydrocephalus before birth, together with general remarks on monsters. In the same, for 1824. Berlin, 1826. Phys. Cl. p. 121. (50.) Description of a rare case of human twins, together with preliminary general observations on twin animals. In the same, for 1825. Berlin, 1828. Phys. Cl. p. 45. (51.) On the absence of particular portions in otherwise perfect organisms. In the same, for 1826. Berlin, 1829, Phys. Cl. p. 83.

H. Mixed Natural Historical and Medical Writings.—(52.) Remarks on Natural History, Medicine, and the Veterinary Art, collected during a tour through portions of Germany, Holland, and France. Berlin, Part 1st, 1804 ; Part 2d, 1805. (53.) Swedish Annals of Medicine and Natural History. Vol. I. Berlin and Stralsund, 1800. Part 1st, 1799 ; Part 2d, 1800. (54.) General view of Swedish Medical Literature for 1799. In Pfaff, Scheel, and Rudolphi's *Nordisches Archiv für Naturkunde, Arzneiwissenschaft, und Chirurgie*, Vol. II. Part 2d. Kopenhagen, 1801, No. 3, p. 79. (55.) General view of Swedish Medical Literature for the years 1800 and 1801. In the same, Vol. III. Part 3d, 1803, No. 1, p. 3. (56.) Pfaff and Scheel's *Nordisches Archiv für Natur-und Arzneiwissenschaft*, Vol. I. (Parts 1, 2, and 3), *Kopenhagen*, 1799, 1801, with one Plate. Pfaff, Scheel, and Rudolphi's *Nord. Arch. für Naturkunde, Arzneiwiss. und Chirurgie*, Vols. II. III. IV. (3 Parts) 1801, 1805, with five Plates. Pfaff, Scheel, and Rudolphi's *Neues Nord. Arch. für Nat. Arzn. u. Chir.*, Vol. I. (Parts 1 and 2), *Frankfurt a. d. Oder*, 1807. (57.) Reviews in the Jena, Halle, and Leipsig Literary Gazettes, from 1800 to 1810. (58.) Anatomical and Physiological articles in the Encyclopædia of the Medical Sciences.

I. Translations.—(59.) On the species of seal found in Sweden from C. P. Thunberg's *Beskrifning pa Svenske djur. Upsala*, 1798, p. 85. (60.) A. J. Retzius' Distribution of the Mineral Kingdom. Leipsig, 1798.

K. Non-scientific Writings.—(61.) Biography of Peter Simon Pallas, Berlin, 1812. With a likeness of Pallas. (Reprint from the contributions to Anthropology and General Natural History.) (62.) *Index numismatum in virorum de rebus medicis aut physicis meritorum memoriam percussorum. (Gratul. honores doctorales decem ante lustra acceptos Ch. Knape). Berolini*, 1823. *Cum* 1.

tab. aen. (effig. Ch. Knape in nummo). (63.) *Index numismatum in virorum de rebus medicis vel physicis meritorum memoriam percussorum. (Physiophili Germanici gratul. diem semisecularem, J. F. Blumenbach). Berolini,* 1825, (*ed. II.*) *cum.* 1. *tab. aen: (effig. J. F. Blumenbach in nummo.)* (64.) *Rencentioris ævi numismata virorum de rebus medicis et physicis meritorum memoriam servantia collegit et recensuit. Berolini,* 1829, (ed. III.) (65.) Poems. Berlin and Greifswald, 1798.

Observations upon the Cause which produces the speedy Melting of Snow around Plants. By M. MACEDOINE MELLONI.*

IN one of the last numbers of the Annals of Science of Lombardy, we find some extended remarks concerning the greater or less rapidity of the melting of snow in the country, according to its position, as around trees and bushes, or in open fields, under long herbage, or where dry leaves and other bodies may be placed immediately over it, or suspended at a certain distance above it. The author of these observations, M. Ambroise Fusinieri, alleges that many of the phenomena are quite opposed to the consequences which should result from the laws of radiated heat, as understood by philosophers.† This opinion may perhaps be true, if the results of my experiments upon the different kinds of heat are disregarded; but if these be adopted, the objections of M. Fusinieri fall to the ground of themselves, and the explication of the observed phenomena becomes nothing more than a simple and pure application of the properties of radiated heat as now established.

We shall first attend to the observations and reasonings of the author: and, that I may present them as distinctly as possible, I shall take from them every thing that is extraneous to the subject now especially before us, and shall present them in the order which to me appears the most natural.

In attentively examining what occurs around plants in a hard winter, we readily perceive that the snow which is placed near the trunks of trees and tufts of bushes, melts more quickly than at a certain distance, so that there is speedily formed around these bodies, in the bed of snow which covers the earth, excavations which are more or less hollow superiorly, and more or less deep. This effect, in favourable circumstances, is very conspicuous. Among other instances, M. Fusinieri cites the winter of the year 1830, in which the ground in Lombardy was entirely bare around trees and shrubs, whilst the snow remained two and a half feet thick in the middle of the fields.

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* Bibliotheque Universelle, June 1838.

† Annali delle Scienze del regno Lombardo-Veneto, Opera periodica di Alcuni Collaboratori, Gen. et Feb. 1838, p. 38.

Nothing is easier than to prove that the cause which produces this speedy solution is not a heat which is peculiar to the plants in their living state, for precisely the same phenomena are observed around poles and stakes which are fixed in the soil.

Snow is also melted by the action of branches and twigs which are situated above it. In fact, all the soil which is immediately beneath trees and bushes, as well as a limited space around them, is cleared before any other part of the surface of the ground.

To demonstrate that this effect is owing to the calorific action of the branches, and not to the smaller quantity of snow which lies there, you may suspend dry branches or those which have been lately cut, at a certain height above the surface, in the midst of a plain which is covered with snow, and you will find, in these circumstances, in which the snow is to a certainty of equal thickness, that precisely the same phenomena occur; that is to say, that beneath these bodies there is speedily formed, at the surface of the snow, hollows, which gradually extend in breadth and in depth, and which would penetrate to the soil, if the experiment were sufficiently prolonged.

Other circumstances being equal, the action of plants is greater in proportion as the twigs and branches are more numerous and slender. It commences at noon, and then progressively extends, to the west, to the east, and finally reaches to the lateral portions of the snow which are situated to the north of the tree. Hence it may be deduced, that the principal cause of the phenomenon arises from the solar heat which is directly communicated to the trunks and branches of trees, and thence radiated upon the surrounding snow.

And now the great objection of M. Fusinieri occurs. How is it possible, says he, that a body heated by the influence of radiated caloric can produce a greater effect than the direct rays themselves? the heat emitted by the plants must be much less in intensity than the solar heat itself. But, if the events occur as is usually supposed, the very contrary of what we observe should happen, so that in open places, *where only the shadows projected by trees and bushes are thrown*, the snow should disappear more speedily than *in spots completely overshadowed by plants;* and we should no longer have the scientific discrepancy of observing the greatest effect where the cause is least. The explanation, therefore, adds M. Fusinieri, of these facts, by the ordinary theory of radiated caloric, cannot be admitted.

I allow that the melting of snow under the action of radiating caloric, ought to increase in proportion to the energy of the incident rays: I also allow that the direct heat of the sun ought greatly to surpass in intensity the heat which emanates from branches and trunks of trees, which are only heated by its influence. But in maintaining that, in the observed phenomena, the effect is, so to speak, in the inverse ratio of the cause, it would be previously necessary to prove that snow with equal facility absorbs the direct solar rays, and those which are emitted by the heated bodies of plants. Else, if these latter rays are much more readily absorbed than the former, there will be no manner of contradiction, and the less action of the more intense rays will be only a natural consequence of their less ready absorption. The error of M. Fusinieri arises from this circumstance that he still admits, with Leslie and Rumford, a uniformity in the absorbing power of bodies for all kinds of radiating heats, whilst our experiments have demonstrated that these powers are liable to very great changes, when we vary the quality of the calorific rays.

That we might produce a fact every way analogous to that now before us, I freed my thermo-electrical pile of the lamp-black which usually covers it; I then painted it white with carbonate of lead, and, after having supplied it with its small tubes, I shut one side, and caused the light of a lamp, concentrated by a lens, to fall upon the other. The galvanometer, when put into communication with the pile, presently marked a constant deviation of 15°. After this I interposed on the passage of the rays, and quite near to the pile, a sheet of thick paper, of a deep grey colour, and speedily the deviation of the galvanometer was increased, and after a few minutes terminated at 33°.5. Here, then, we perceive a body heated under the action of calorific radiation, producing an effect two or three times greater than the direct rays of the source of the heat itself.* But, after what we have already said, it may readily be understood how this should happen.

Let us divide the radiating heat which directly strikes the thermo-electrical pile into 100 equal parts, and let us suppose that ten of these parts are absorbed, and the rest are reflected. Again, if the interposed sheet of paper, after being itself heated from the source of heat, radiates to the pile only twenty-five parts of the heat, and if, of these twenty-five, there are only five which are reflected, and twenty which are absorbed; then it is clear that the heat communicated by the paper, although more feeble by three-fourths than the heat direct from the source, will nevertheless heat the active side of the pile twice as much, and will consequently produce an action which is twofold more intense.

But it will be here demanded, if snow has really the property, like the carbonate of lead, of absorbing different kinds of radiated heat in different proportions? The following experiments will supply the answer to this inquiry.†

One winter's day, when the temperature was at 2°.5 below zero, the sky being cloudy, the air tranquil, and the ground covered with recent snow, I placed the thermo-electrical pile, blackened as usual, on one of the casements of my window. On its one side I put an argand-lamp, and on the other a bent plate of copper, heated posteriorly to about 400° by a spirit-lamp. By this arrangement, each of the faces of the pile fronted a source of radiating heat, in such a way that the two calorific actions tended to compensate each other. I approximated the feebler source till the corresponding galvanometer maintained itself at the zero of the division.

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* Although we here employed flame, it is not to be concluded that the experiment requires the presence of light; for, in transmitting the calorific rays through black and completely opaque glass, before using them, an operation which very thoroughly disengages them from all concomitant light, the interposition of paper still gives a considerable augmentation in the deviation of the galvanometer. In fact, this obscure radiation, which directly produces from 10° to 11° of deviation, gave from 18° to 19° when it was absorbed by the sheet of dark grey paper, and then transmitted upon the whitened pile. This experiment, which I repeat with the greatest facility to any one wishing to witness it, alone suffices completely to overturn the theories by means of which some wish to account for the present phenomena and other analogous actions, alleging it is an actual transformation of light into heat.

† These experiments upon snow are extracted from a somewhat extended work, which was long ago commenced, upon the absorbing and emissive powers of bodies in general, and which is not yet finished. I publish them in this detached way, because they appear to me to give a complete answer to the question now agitated by M. Fusinieri.

I then took a small copper tube, having the same dimensions as the envelope of the pile, and provided like it with a stalk, by which it might be introduced into the same support. This tube, which was open at both ends, was divided perpendicularly into two equal chambers, and into each of these I introduced pure snow to a height corresponding to about half the length of the thermo-electrical bundle (*faisceau*).

I now removed from the stand or support the pile which we have just described as placed between the argand-lamp and the heated plate of copper, and in its place I substituted the tube filled with snow. Each of the two portions of included snow was thus exposed to the action of a source of heat; and the two calorific radiations, at the places where they infringed upon the corresponding snowy columns, were of equal intensity. But notwithstanding this, the snow column which was contained in the cavity turned towards the plate of copper heated to 400°, melted much more readily than did the other in the opposite cavity. This trial concluded, I again filled the apparatus with snow, and replaced it upon the stand of the pile, taking care, at the same time, now to turn towards the lamp the side which previously fronted the heated plate. Still, however, the melting was accomplished much more rapidly on the side of this latter source of heat; and so it was every time, and as often as I continued the experiment. The mean time for the total disappearance of the snow on the side of the lamp was about nine minutes and a half, whilst on the side of the heated plate, the mean time did not exceed four minutes.

This experiment proves, in the most satisfactory manner, that the calorific rays from different sources are absorbed in different quantities by snow as well as by carbonate of lead. I shall now add two other observations of the same kind, which do not require the aid of the thermo-multiplicator, and which bring out facts which are sometimes identical, and sometimes diametrically opposed to those which have been pointed out by M. Fusinieri.

Having filled, and more than filled, a cylindrical vessel with fine and newly-fallen snow, I removed all above its rim, by means of a wooden ruler, so that its surface was a very uniform plane of snow. I then turned this plane vertically, and upon it I caused the rays of an argand-lamp to fall, after having suspended before its central portion, and quite near to the surface of the snow, a small disk of thin pasteboard, having both its sides thoroughly covered with lamp-black. The rays of the lamp, of course, darted partly on the disk and partly on the snow. In a very short time the plain surface was hollowed out beneath the disk, and at the end of a quarter of an hour this cavity was not less than three or four lines deep at its centre.

I next placed the apparatus in the same circumstances it was at the commencement of the previous experiment, with the difference of substituting the copper plate at 400° of temperature for the lamp. The phenomena then presented themselves quite in the inverse way, that is to say, the melting or corrosion of the snow was more abundant, where the direct rays impinged, than in the part situated opposite the disk, so that a protuberance, instead of a hollow, was very soon formed in its centre. Hence it follows, that a certain energy of the incident heat is not sufficient alone, to produce a greater action upon that part of the surface which is shaded by the disk; there is, moreover, necessary, that peculiar quality of calorific radiation which is analogous to the solar heat, and which, like it, is ordinarily accompanied with luminous radiation, but which does not require it as a necessary concomitant.*

If the reasoning, which we have connected with the experiment in which the grey paper was interposed before the white painted thermo-electrical pile, is accurately understood, the explanation of these differences in the melting will not present any difficulty.

In the former case, the heated pasteboard darted towards the vessel, rays which were much more absorbable than the direct rays from the source of heat: hence it followed that the quantity of melted snow is greater in the part shaded by the disk than elsewhere, in spite of the smaller quantity of heat which reached it. In the second case, again, where the source of heat, and the pasteboard heated by its influence, furnish rays which are nearly in an equal degree absorbable, the disk only diminished, by being interposed, the effect of the direct radiation, and thus rendered the melting less in the shaded portion.

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* See Note 2, p. 255.

From all this we conclude, that the speedy melting of snow around plants, instead of being found in opposition to the existing theories of radiated heat, as M. Fusinieri alleges, is, on the contrary, only a very simple consequence of it.

It will probably be expedient to subjoin a few additional remarks, to account for the more minute details of this phenomenon,—details which are easily explicable, when we start from the principal fact, and take some accessory circumstances into consideration. If, for example, it be demanded, why, beyond the power of the solar rays, the elevated temperature of the air contributes to accelerate the speedier melting of the snow around trees and solid bodies generally, standing in the plain, the cause is easily found in the obstruction which these bodies offer to the direct radiation from the snow towards the celestial spaces; this maintains them near the temperature of fusion; whilst the snow which is lying in exposed places is cooled down many degrees below zero, in virtue of nocturnal radiation, and is consequently much less disposed to melt under the action of the ambient medium. With the same facility we may explain why the influence of plants is still conspicuous when the sky is entirely covered with clouds, and the temperature of the air below zero, for the diffused heat of the sun possesses absolutely the same properties of transmission and absorption, as the direct heat, and ought, consequently, to produce effects wholly similar, so far as the intensity is concerned.

In considering the action of prolonged calorific radiation upon a series of bodies endowed with the same absorbing powers, it will be seen that those which possess a smaller mass of matter should be heated more speedily than the others, and arrive at that degree of heat which the state of the surface, the power of the incident rays, and the pressure and temperature of the air, will allow; and upon reflecting that the influence of solar heat, whether direct or diffuse, continues during the whole day, we here discover the cause of the greater or less degrees of melting which are produced around stakes, &c. of different sizes, and which, far from being in proportion to the mass of matter, as would occur if these substances were heated to the same temperature previous to their being implanted in the snow, follow, on the other hand, within certain limits, the inverse ratio of the diameters.

But here we enter upon the development of theories which have long been known; and the object of this communication was to submit to the judgment of natural philosophers a particular explanation of certain general principles which have only recently been introduced into the science of heat.

Notice of a Dioptric Light erected at Kirkcaldy Harbour, with Description of the Apparatus for cutting the Annular Lens to the true optical figure.[*] By Edward Sang, Esq. F.R.S.E., Civil Engineer, Edinburgh.[†] Communicated by the Society of Arts.

The harbour of Kirkcaldy, like the greater number of harbours on the coast of Scotland, is tidal, being left completely dry even at the ebb of neap tides. The larger class of vessels which frequent the port, can only enter the harbour at or near the stream; and thus the increased commerce of the place had rendered it an object of some importance to have the entrance thoroughly lighted.

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[*] Read before the Society for the Encouragement of the Useful Arts in Scotland.

[†] Report of the Committee to whom it was remitted to consider of Mr Sang's paper, relative to a Dioptric Light erected at Kirkcaldy Harbour. Read 25th April 1838.

Mr Sang's invention of grinding annular surfaces of any form by means of cutters attached to a moveable arm, whose end is guided by a spring uncoiling itself from the evolute of the curve surface which the lens requires, is novel and ingenious; and if equally applicable to the construction of instruments requiring great accuracy of form, promises to be extensively useful. The mode of giving any required direction to the scratches or small indenta-

The harbour Commissioners, having in the summer of 1836, resolved to place a light at the east pier, my brother, Mr John Sang, suggested to them the propriety of surrounding the intended gas-burner with an annular lens, so as to render useful the light that otherwise would have proceeded upwards. Having, however, felt some doubt as to the possibility of constructing a lens of this kind on so small a scale, he consulted me on the matter, and the ultimate result was, that I undertook to supply the lens.

My object in undertaking, at that time, such a task was twofold. In the first place, I was desirous that no difficulty either in expense or in workmanship, should prevent such a benefit to the harbour; and in the second place, having been engaged in a long series of experiments on the art of cutting, and having arrived at what I conceive to be some general principles, I was willing to regard the formation of the lens as one of those experiments, or rather as a kind of test of the truth of the detected laws. The entire success of the attempt, has exhibited the possibility of turning or planing glass and of polishing it, to almost any required figure, and that with a degree of precision sufficient for many optical purposes.

The annular lens invented by M. Fresnel, and applied by him to the *Phares* of the French Coasts, as also by Mr Alan Stevenson to some of our lighthouses, is a solid of revolution

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tions made in the process of grinding, is very simple; and consists partly in reversing the motion of the cutter, or of the chuck on which the lens is placed, and partly in altering the ratio of the velocities of the surfaces in contact. Any degree of obliquity in the direction of these scratches may in this way be produced, both from right to left and from left to right, and thus every possible variety in their direction must be the result; so that the whole effect ordinarily produced by crossing the motions in the usual grinding process, may be obtained. One would, therefore, be induced to expect great accuracy from this method; and Mr Sang has certainly succeeded in giving to the Kirkaldy apparatus, a very fine polish, which is a matter of great importance.

We consider Mr Sang's labours as important in regard to the manufacture of Lighthouse apparatus, and as calculated to improve the manufacture of refracting instruments generally; and we would, therefore, beg leave to recommend that his communication be made known to the public.

Robert Stevenson.
Alex. Adie.
William Galbraith, *Convr.*

Edinburgh, *June* 20. 1838.

generated by turning the section of a common lens round a line passing through the focus, and perpendicular to the axis of the common lens. The focus is thus in the interior of the annular lens, and the rays proceeding from it, instead of being converged to a conjugate focus, are flashed out horizontally. On the large scale, these lenses are built of many zones, but, in the case in hand, only one piece of glass was used; and the nicety was this, to give to the surface of the small lens that variation of curvature, which is attained on the separate zones of the large lenses.

Three classes of difficulties presented themselves, first, the detection of the proper curve; second, the manufacture of the lump of glass; and third, the cutting and polishing it.

The first difficulty belongs to applicate geometry; and the detail of the method of resolving it, would be here somewhat out of place. It may be enough to notice, that, the cylindric form having been determined on for the interior surface, on account of the facilities which it promised in the manipulation, the outline of the exterior surface necessarily became a curve of high order. The accompanying drawing shews the section of the lens full size; as it resulted from very laborious calculations.

The form of the lens being thus obtained, the next business was to procure the glass, and here obstacles presented themselves much greater than I anticipated. My first idea was to use flint-glass on account of its high refractive power, but after many attempts, of which the most successful result is presented to the Society, I abandoned that idea, and fell back upon crown-glass. The Messrs Cookson of Newcastle, furnished me with two pieces, which reached me entire, but one, and unfortunately the better, had received a blow on one of its edges, and a tendency to split shewed itself soon after commencing operations: it also accompanies the paper. The other was perfectly sound. On account of the lower refractive index of crown-glass, the lens was carried to less height than had it been of flint-glass.

My first business was to bore out the cylinder. For this purpose, I fixed a tin plate on the point of my drilling spindle, and having primed the edge of it with diamond powder, I cut out a series of grooves parallel to the axis: the ridges between these were removed by using intermediate stops on the lathe-spindle; and the whole was then smoothed out by a cylinder of lead with fine emery: it was then ground and polished in the usual way for hollow cylinders.

In order to cut the outside to the proper form, the lens thus bored out, was chucked on a turned block of lead, which had been cast on an iron mandril; the surface of the lead being previously covered by a fine thread which was nowhere doubled. By this means the axis of the interior surface was made coincident with that of the lathe.

The evolvent of the required curve was then computed, and the edges of two plates of cast-iron carefully formed to it. These are seen attached to the cheeks of the accompanying frame, and are represented in the drawing. Two pieces of watch-spring were then made to bend over them, so that on uncoiling the springs, one point in each would trace over the curve wanted: these springs were then attached to the ends of an axis by adjusting pieces, and that axis had its parallelism preserved by means of a jointed frame.

This apparatus so prepared, was fixed on the bed of the turning-lathe, and the moveable axis rendered perpendicular to the axis of the lathe. On this moveable axis there were placed the cutters and polishers; it received motion by means of a small pully fixed on it near one end.

To make the first approximation to the shape of the lens, an iron cylinder was fitted on the moveable axis, and its surface was primed with diamond. The lathe-spindle being still, this iron cylinder was brought over the glass, so as to cut part of a cylindroid surface, whose base was nearly the required curve; the lathe-spindle then being moved a division round, another surface was cut, and this was continued all round; as soon as a sufficient approximation was made, both spindles were set to revolve at once, and the cutting continued till a uniform surface was produced. The iron tool with diamond was then replaced by a tin one with fine emery, and the separate motions were varied and reversed so as to produce every variety in the direction in which the surfaces met each other. Lastly, the tin tool was removed, and its place supplied by one of willow, the surface of which was covered with carefully worked putty; the same motions which had been used in smoothing, were now employed in polishing, greater attention being paid to the frequent reversion of the motions.

It was on this combination of motions, which I relied for producing a true finish. The nature of the action was this: Suppose both motions to be dentral, the point of the polisher would meet the surface of the glass obliquely, the minute scratches inclining to the left, and the degree of inclination depending on the relation between the velocities. Let now the motion of the lathe be reversed, the direction of the scratches is immediately altered, and the one set of traces crosses the other. By varying the velocities, the direction of the scratches was still further changed, so that all those effects were, in this way, produced, which are obtained from crossing the strokes in the usual processes of grinding.

The lens, after being finished, was supported on three brass supports placed edgeways, so as to intercept little of the direct light, and between these was placed a small argand gas-burner with a sliding stalk, so as to be adjustable in height. The lantern within which the whole is placed, is in the form of an octagonal decaedron, its top and bottom being squares placed 45° upon each other, and its sides isosceles trigons. The form was adopted because it contains the essential elements of strength, and because the side astragals being all inclined, would not, from any point of view, intercept a sensible portion of the light. The support of the lantern is a cast from the pattern of our police gas pillars; it is imbedded deep in the mason work of the pier.

The entire apparatus has a very insignificant appearance, and may readily be mistaken for a common street lamp. Notwithstanding its exposed situation during the past nine months, it has met with no accident (except the freezing of the water in the meter), and has afforded a sufficient light at a very trifling cost, and with scarcely any attendance; it is lighted in the evening and extinguished in the morning, and requires no attendance in the interval.

In designing this instrument it had to be kept in view, that the light was not to serve for distant vessels, but merely for those making the port. For this reason, only the upper half of the lens was used, and the lower edge of the bright flame brought into focus. A very copious light is thus thrown on the pier and on the water in the vicinity, so that the seamen can work the landing lines, and the custom-house officer make his entries with ease. The existence of the harbour light has, indeed, nearly doubled the number of opportunities for entering the port.

In order to complete the instrument, a reflective ring ought to have been placed on the top of the annular lens, so as to save some more of the rays that proceed upwards:—the funds placed at my disposal did not allow of that, but I have made provision, on the upper edge of the lens, for securing it there if it should be thought of.

To conclude, I may point out a mistake into which M. Fresnel has fallen with regard to the reflective rings employed by him. He places the focus of both the refracting and reflecting system at the lower part of the bright flame, whereas the focus of the refractors only ought to be at the bottom, the focus of the reflectors should be at the top of the flame.

EDWARD SANG.

EDINBURGH, 13*th April* 1838.

Description of an Experiment regarding the falling out of the head of the Thigh-bone from the Socket in rarefied air, explanatory of the great prostration of strength experienced during the ascent of lofty mountains.[*] By Professor WILHELM WEBER of Göttingen.

Alexander Von Humboldt delivered an address to the Association of Naturalists at Jena, at the public general meeting of the 26th September, in which he described his ascent of Chimborazo, and compared it with the subsequent one of Boussingault. Among other interesting subjects, he alluded particularly to the remarkable feeling of fatigue experienced while walking in very lofty regions; and remarked that this curious phenomenon may probably be explained by means of the equilibration of the bones produced by the pressure of the atmospherical air, and which has been pointed out by my brother and myself, in our work "On the Mechanics of the Organs of the Human Body" (*Mechanik der Menschlichen Gehwerkzeuge, eine anatomisch-physiologische Untersuchung von den Brüdern Wilhelm Weber, Professor in Göttingen, und Edward Weber, Prosector in Leipzig. Mit* 17 *Tafeln. Göttingen*, 1836.)

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[*] From Poggendorff's Annalen. 1837. No. 1, p. 8.

We have, in the above-mentioned work, proved by direct experiments, that the weight of the bone, when attached to the trunk, and which amounts to about twenty pounds, neither hangs on the muscles or ligaments, nor even rests on the edge of the socket, but is supported by the pressure of the air, which squeezes the two surfaces of the joint together. By means of this equilibration of its weight, the bone acquires as perfect a power of turning in its socket as is necessary for the performance of such active movements as walking and running. If, then, the pressure of the air becomes diminished, a point must be reached, when that pressure can no longer preserve the equilibrium of the weight of the bone. Another power, such for example as that of the muscles, must now take its place and support the bone; as, otherwise, the two surfaces of the joint would remove from each other. It is however to be expected, that, when the bone is supported in this less advantageous manner, which not only causes an expenditure of strength, but also obstructs the movements of the bone, owing to the stiffness that is induced in the muscles called into action, derangements and inconveniences should take place in walking, which would not occur if the bone were kept in equilibrium by the pressure of the air.

In very elevated regions, where the pressure of the air is reduced almost a half, such inconveniences really occur, viz., so great a degree of fatigue of the bones is perceived, as would lead us to suppose a derangement of the mechanism employed in walking, and which is only felt so long as we walk, is removed by sitting down, but again produced by walking onwards; and on this account Humboldt requested us to make an experiment with the air-pump, by which the sinking down of the head of the thigh-bone under such circumstances might be made apparent, and which might at the same time decide if the amount of rarefaction of the air, which takes place on high mountains, be such that the diminished pressure may be no longer sufficient to support the weight of the bone, and if we should thus be justified in attributing the fatigue to such a derangement of the mechanism of the organs of motion.—We have really performed this experiment during our residence in Berlin.

A fresh pelvis, together with the thigh-bones, for which we were indebted to Professor Schlemm, was divided through the *os sacrum*, and the fragments of the pelvis, as well as the thigh-bones, so cut as to admit of the hip-joints being easily suspended under the receiver of an air-pump. Perpendicularly above the hip-joint a hole was bored through the bone of the pelvis, and a cord passed through it, in order by that means to hang up freely the hip-joint. A second hole was bored perpendicularly under the joint through the thigh-bone; so that a weight might be attached to it, which should come in place of the bone that had been cut away.

After having made these preparations in the laboratory of Professor Magnus; and assisted by him, and in presence of Professor Müller and my brother, I performed the following experiment:—

On the one hip-joint, the capsular membrane was cut through close round the thigh-bone, so that it no longer united the two bones with each other. Those present convinced themselves that this had really completely taken place, and still the two surfaces of the joint were not only in perfect contact, but (by the pressure of the air) were held fast together. Afterwards the bone of the pelvis was secured by the upper cord to a hook in the cover of the receiver. The apparatus was thus suspended freely from the cover of the receiver, and high above the plate on which the receiver was placed. To the thigh-bone a heavy weight of two pounds was attached by a cord. The height of the lowest hanging weight above the plate was measured by Professors Müller, Magnus, and Weber; and then the air was withdrawn from the receiver, till its pressure was diminished to three Parisian inches of mercury. By this the blood was driven out of the vessels of the bones, but the head of the thigh-bone, together with the attached weights, preserved their original position. Scarcely, however, was the pressure diminished below three inches of mercury, when the head of the thigh-bone began to sink gradually, and to the amount of half an inch, which was as far as was permitted by the capsule. For the capsular membrane forms a ring round the neck of the head of the thigh-bone, which is smaller than the circumference of the head. This ring, therefore, allows the sinking down of the head; but it prevents it from escaping altogether: that is, it protects the hip-joint from dislocation.

After the head of the thigh-bone, in consequence of the rarefaction of the air, had fallen eight lines (as far as it was permitted by the capsular ring) we allowed the air to re-enter the receiver. Immediately the fallen head of the thigh-bone, with its attached weights, rose, and was elevated rapidly in a palpable manner,—apparently of its own accord, but in reality in consequence of the pressure of the air, which had entered the receiver but not the socket,—until it reached the cover of the receiver, or, in other words, had returned to its original height. This experiment was repeated several times, by alternately exhausting and readmitting air to the receiver, and always with the same result.

The experiment was next performed after the capsular ring had been cut transversely across, and the head of the thigh-bone had been even entirely taken out of the socket, yet it was again pressed in with violence. In fact, the air thus forced into the socket became so much compressed that the head of the thigh-bone was again suspended by the pressure of the air *from without.* (Our previous experiments proved that the hip-joint may be regarded as an air-pump, out of which the piston does not fall by its own weight, even when some air is left. This air becomes very much rarefied whenever the piston falls in the slightest degree. We bored a fine hole through the ball of the socket, and immediately closed it, opening it again immediately, to further the object of our experiment, but we could not prevent some air remaining behind in the hole after the closing of it, whence it might be distributed, as out of the space in the air-pump where air remains behind, without our experiment being essentially disturbed.) By the diminution of the pressure of the air, the head of the thigh-bone, on this occasion, fell out entirely and more speedily, while previously when the pressure of the air was undiminished, the weight o the thigh, together with the attached weights, could be safely supported.

We had intended to perform this experiment on the other hip-joint in a still more modified form. It turned out, however, that in taking out the joint the *incisura acetabuli* had been too much exposed, so that the air had penetrated into the fat and cellular tissue contained in it, when we loaded the thigh-bone without the receiver; so that the weight even in the open air separated somewhat the surfaces of the joint from each other. Those present, however, convinced themselves that these surfaces were closely drawn together, and could only be torn asunder by great violence, whenever we covered the *incisura* with the finger, and thus kept off the pressure of the air.

These experiments not only confirm generally the accuracy of Humboldt's supposition, but may also serve more particularly to decide, if the rarefaction of the air on high mountains be sufficient to produce this phenomenon. For they have proved that about two and a half pounds (the weight of the cut off thigh bone, and the attached weight of two pounds), supported when the barometer marked three inches, could no longer be supported when there was a small diminution of the pressure. If we reckon the weight of the whole bone at twenty pounds, that is eight times greater, then a pressure of twenty-four inches quicksilver would be sufficient to support the whole bone. If, therefore, the barometer sinks below twenty-four inches on high mountains, so during walking the muscles of the bone that is raised, and is swinging above the ground, can no longer be idle, but must be so exerted, that for every inch the mercury sinks, they must carry five-sixths of a pound *additional.* In consequence of this unusual continued straining, the muscles will not only become fatigued, but, as this straining is opposed to the swinging which has to be performed by the bone, a feeling of uneasiness and inconvenience occurs in walking, which seems to explain the described sensation of fatigue.

It deserves to be remarked, that in spontaneous lameness,—where the bone is no longer held in its place by the pressure of the atmosphere, but even in the beginning of the disease, sinks so far out of the socket, as is permitted by the capsular membrane—also a speedy and considerable fatigue is perceptible in walking, which often, together with the lengthening of the bone, is almost the only sign by which the commencing complaint is made known externally.

On an Arrangement in the Construction of the erecting of Achromatic Eye-pieces of Spy-glasses, whereby their magnifying power may always be adapted to the state of the atmosphere. By CHARLES GORING, M.D. Communicated by the Author.

THE artists of Vienna and Paris having adopted the principles laid down by me for making the eye-pieces of spy-glasses, *bona fide achromatic*, as originally given many years ago in a paper in the Edinburgh Journal of Science, and afterwards more fully developed in a tract in the "Micrographia,"* I avail myself of the circulation this journal has on the Continent to suggest an improvement upon *this description of eye-pieces*, which will, I think, be found highly convenient and useful;—(*to those of the common construction it cannot be applied with effect and advantage*).

They who are acquainted with my writings will not require to be informed that the doctrines I have promulgated on the construction of achromatic erecting eye-pieces are, that they are neither more nor less than compound microscopes, applied to the purpose of magnifying the image of the primary object glass, and that if they are to be *really* free from chromatic and spherical aberration they can only be made so by the operation of concaves of flint glass applied to the secondary object-glass, or that of the compound microscope. It is well known that spy-glasses are made of much greater length than can be conveniently held and directed by the hands, and that, in consequence, if their power is considerable, the object dances on the retina in such a manner that it is impossible to examine it, and the goodness of the glasses is thus nullified in practice. My object has been to shorten them by combining two primary object-glasses together, to double their angular aperture while their focal length is reduced one-half, thus rendering them dumpy, short, and thick. If this is done, the necessity of adopting erecting eye-pieces of my construction, will very soon be recognised even by an English optician. As to increasing the angular aperture of the primary or telescopic object-glass, I only say, that *if it is well executed*, its defining power will be increased, as will be shewn by its giving a much smaller spurious disc to a star than one of a longer focus and smaller angle of aperture will; and if it is composed of two double object-glasses with their inner curves cemented on the French plan, it will be more luminous than an ordinary triple one.

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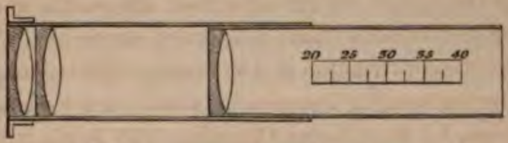
* On the chromatic and spherical aberration of eye-pieces.

I here give the construction of a secondary object-glass, or that belonging to the erecting eye-piece as made on my plan, and at my instigation, by M. Charles Chevalier, 163 Palais Royal, Paris; and if it is thought that it consists of too many glasses, and that the quantity of light lost will be very great, I can safely affirm that whoever tries it, provided the convex lenses are made of fine French plate-glass, will be agreeably surprised at its luminousness. With an object-glass of two French inches of aperture we may have a power of 40, with sufficient light, which I should say is as much as can be used advantageously upon any *distant* objects, even in a much clearer atmosphere than that of the British Isles. I may observe that the said secondary object-glass may be made of two double achromatics instead of three, but then the edges of the field of view will not be quite so good, *if it is a large one at least.*

My readers have only to unscrew that part of an erecting eye-piece which forms the second image, consisting of two glasses, usually called the third and fourth, with a stop having a very small perforation in it, which is placed between them, and to suppose two double cemented achromatics substituted for the fourth, and another for the third having the same foci and apertures with the common glasses, the stop being removed, and they will arrive at a perfect idea of my construction. That M. Chevalier has executed for me, has the same dimensions, foci, &c. as the secondary object-glass of a 20-inch day and night glass; and the improvement I wish to suggest upon it consists merely in placing the third glass in a sliding tube, so that it may either act positively or negatively, either in contact with, or at a distance from the fourth, for in this way the power of the telescope may be augmented or decreased one-half, and changed, say from 40 to 20 or from 20 to 10, or the reverse, and every intermediate change of power, between 40 and 20 or 20 and 10 &c. may also be obtained at pleasure, to suit the state of the weather. It will be necessary, however, to accomplish this purpose that the two double achromatics should be *perfect in themselves*, and the single moveable one also, otherwise the balance of aberration will be liable to be disturbed by a change of position. There is no stop in this construction to keep out false light, for it would be necessary to alter its aperture every time the magnifying power of the glasses was changed, otherwise it would be ineffective, or cut off something from the aperture of the primary object-glass; therefore, it will be necessary to furnish the main tube of the telescope with stops at every joint, embracing the cone of rays from the first object-glass, pretty closely to within a few inches of the eye-piece, which will preclude the necessity of a stop between the glasses, and be of the greatest use when we view objects while the sun is low down in the horizon, and nearly opposite to us. If stops must be had between the said glasses, a wheel similar to that carrying the concave eye-glasses of a perspective may be placed directly before the third glass, with holes in it corresponding to the size of the image of the first object-glass as given by the different magnifying powers, caused by pulling out or thrusting in the sliding tube; and when the observer has fixed upon his power, the hole adapted to it must be rendered centrical by turning the wheel round. In order to render the spy-glasses still more capable of being directed with firmness and precision towards an object, and of preserving it steadily in the middle of the field of view, I have recommended M. Chevalier to cause them to be fastened at pleasure to a slight stock like that of a gun: two straps with buckles will secure the glass as well as anything; and this would be no bad appendage to any spy-glass not of an immoderate length.



It must be obvious that when a system of glasses, such as I have described, is made of long focus, it must form an excellent *Megalascope*, or glass for viewing the larger kind of microscopic objects, such as minerals, flowers, assemblages of the aquatic larvæ, &c.: and, accordingly, the double glasses of mine having a combined focus of 2¾ inches, and the single one 3¼ inches, form an admirable instrument of this sort, both separately and in combination. Thus the single one *in situ* may be used by itself by unscrewing and removing the double ones, which may again be employed separately by removing the single one; lastly, the three may be used combined, either at a distance or in contact (as in the eye-piece), giving a great variety of powers.

There is an effect produced by *shading* object-glasses *when of long foci and large aperture*, which I do not very well comprehend. It is not observable in those of short foci. Thus the single achromatic of 3¼ inches focus and ⅞ inch of aperture acts most beautifully *when in situ shaded by the projecting tube*, but if freely exposed, requires to be cut off to half an inch of aperture to act as well. The same may be said of the two which are combined in contact, whose performance is vastly improved by a shade,—though it does not in any way reduce their aperture.

When all the glasses are combined, the single one at a distance and acting negatively, the power being a minimum, if a stop of half an inch of aperture is inserted immediately behind the combined double achromatics, *it will in no degree reduce the size of the visual pencil which enters the retina*, but greatly improve the effect of the combination. Whether it affects the *penetrating power* of the object-glass or not, I have not been able to ascertain as its focus is too long to shew lined test objects. If this arrangement should be found innocent as regards penetrating power, it ought certainly to be adopted in object-glasses of moderate power. There is another lesson I have learnt from the study of these large achromatics (which of course give an immense visual pencil with a shallow eye-glass), viz., that the *iris* does by no means act as a *stop* with regard to optical instruments, or render inert and of no effect all that portion of a visual pencil which exceeds its own diameter (for as the visual pencil of a microscope or telescope is nothing but the image of its object-glass or metal formed by the glasses next the eye), at this rate the iris should be equivalent in effect to a circlet placed over the objective end of the instrument, or an eye-hole of its own aperture, which I am sure is not the case.—*June 9th* 1838.

Theory of Granite, and the other massive Rocks, together with that of Crystalline Slate; proposed in Lectures on Geology, in the University of Christiania in Norway, in the year 1836. By B. M. Keilhau, Professor of Mineralogy. Concluded from page 101.

At the commencement of the transition period, the primary series of rocks, with their highly inclined strata, had already a surface exactly like that portion of it which is at the present day exposed to our view. On this surface were deposited, under the covering of the ocean, the materials for clay-slate, limestone, and sandstone; and over a great extent, certainly a somewhat greater extent than the now existing transition districts, the primary rocks were in this manner concealed by newer stratified masses. These afterwards underwent disturbances, we know not in what manner, which produced the high inclination that prevails with remarkable regularity throughout the greater part of our transition districts. Then, large portions of these newer stratified tracts began by tranquil processes to become converted into massive mountain-rocks; portions, in which we should otherwise now have seen clay-slate as the prevailing rock, converted into granite and syenite, and sandstone portions into porphyry. Nothing is more apparent to the eye than the first-mentioned kind of metamorphosis, in which a gradually diminishing granitification can be traced into the fossiliferous strata; and where, on the other hand, sharp boundaries and felspar ramifications in the slate, operate, with those individuals who do not remember that analogous phenomena occur in the case of dolomite, against the conviction obtained at the localities mentioned above, yet still that conviction is again strengthened by the consideration of the large and small masses of slate which are left in an undisturbed position in the crystalline mountain-rock. A phenomenon which we did not touch on in our previously given volcanic representation, viz. that small, altogether isolated, granite developments* occur at the sides of large masses, confirms likewise the same view.

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* During my stay in Saxony in the year 1825, I observed analogous but much less perfect developments in the limestone which protrudes from be-

It is quite evident that the processes which have caused the formation of granite belonged especially to the clay-slate tracts; but as these every where contain masses of limestone, we find that the limestone was also changed in those regions where granitification took place. In the same manner, in particular places, the sandstone may have been transformed, together also with portions of the primary strata, into which we have found that transition-granite penetrates, partly as forming a passage to the fundamental gneiss, and partly ramified in it. In the granitic formations are contained several chemical constituents,

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neath the syenite at Weinböhla, between Dresden and Meissen; these were partly small, externally blackish grains of a felspathic nature (also remarked by Professor Weiss, but naturally with an entirely different interpretation), and partly small, also imbedded portions, having the aspect of weathered, very imperfect granite. I have lately read, that at other places small fragments of granite have been remarked in the stratified rocks that are associated with the granites and syenites in those parts of the valley of the Elbe. These localities undoubtedly merit the new investigations which are perhaps now in progress, and which I am convinced will produce a rich harvest for the science; as it really seems quite acknowledged that the connection of the groups of facts hitherto adduced cannot be at all explained.

(Professor Keilhau then quotes Dr Cotta's paper on the relative ages of the granite and chalk in Saxony, from which we translate the following passage: "Near Hohnstein small fragments of granite have been found in a conglomerate-like sandstone, probably belonging to the Jura formation, and which is interposed in an inclined position between granite and sandstone. These fragments, from their mineralogical characters, would seem to have been derived from the same granite which is immediately superimposed. Thus the whole matter is rendered inexplicable; for as, on the one side, we are irresistibly forced to regard the granite as the newer formation, so, on the other, we cannot understand how fragments of this granite have found their way into the subjacent sandstone, if the former be really newer than the latter. The questions, Do these pieces of granite perhaps belong to another older formation? Or is the conglomerate-like sandstone, which contains them, a product of the friction, and only formed by the elevation? seem to include the only, though much sought for, modes of solving the difficulty; for the view proposed by Weiss of the dry elevation of the previously existing and solid granite, which may readily be placed in connection with this phenomenon—though it is ingenious, and at first sight satisfactory—involves many difficult considerations."—*Cotta in Leonhard's Jahrbuch for* 1836, p. 23. Dr Cotta, in his "*Geognostiche Wanderungen,*" has recently expressed an opinion favourable to the idea of the elevation of the Hohnstein and Weinböhla granite in a solid state.—Edit.)

that either do not occur at all, or not in sufficient quantity for the newly formed minerals, in those masses which have undergone the metamorphoses; whereas the latter consist partly of substances of which at least not much (carbonate of lime has been noticed in granite) is discovered in the changed rock. In what manner this exchange is to be conceived, it is for the present impossible to explain more precisely; but I further regard it as much more natural to suppose that no new material was transported from without to produce the converting action to which such regions were subjected, than to assume that there were really cases where it must be imagined that sublimations of potash, silica, &c. actually came from the deeply-seated volcanic laboratory.

The great porphyry formations occur principally in the sandstone, which forms the deposit above the clay-slate and limestone. It is probable that the action took place especially on the same fine-grained partly earthy sandstone-formation, often almost passing into iron-clay (Jernleer), of which there is still much remaining in an unaltered state, both in the porphyries and in other situations. When the sandstone had the character of conglomerates, there thus arose, at many points at least, a resistance to the transforming action: in this manner we often find certain layers filled with small quartz pebbles, directly under those porphyry masses, whose development in the uppermost sandstone *etage* was interrupted by these coarse strata. Beautiful proofs of the tranquil formation of the porphyry are afforded by the strata as well of the clay-like sandstones, as of the conglomerates, for these stretch far into the porphyries, having the same gentle inclination which is so general in the sandstone strata. Particular strata also, which are now fairly included in the massive mountain-rock, stand partly in connection with that sandstone basis, from whose range of strata we can follow a long portion projecting like a plank, and can at the same time convince ourselves that it is not in the slightest degree broken or bent; but even when the connection with the body of the strata is broken off by the passage into the porphyry which also takes place, we can in like manner ascertain distinctly, by the angle of inclination, the unaltered position of the portions of strata, still in the form of sandstone, lying in the porphyry. This fact was not touched on formerly by us; it likewise belongs to those which volcanists cannot perceive.

Porphyry, with its more or less uncrystalline basis, does not appear to have been so much adapted as the granites and syenites to form thin branches proceeding in every direction into the bounding strata: continuations of the principal mass of the porphyry are certainly met with as veins situated in the subjacent strata, but these veins are so large, and of such solid forms, that they cannot be at all compared to the multitudes which ramify from the granitic masses.

The more subordinate crystalline siliceous rocks occurring in the stratified transition series, were probably developed at the same period, and in the same manner, as those larger masses. That the transforming action operated especially on certain beds, and that the nature, and perhaps also the position, of these beds had a particular influence on the metamorphic rock which made its appearance, seems extremely natural; and I now allude particularly to eurite porphyry, which so often occurs in the form of beds. But as to deviations from occurrence in the form of beds, there is nothing very peculiar, when we take into consideration dolomites, granites, &c., and are accustomed to observe the readiness with which it appears that the processes have spread around, and with which they have taken particular directions during their progress through the masses encountered; —a readiness which, however, seems limited in other cases, inasmuch as metamorphoses at particular points have evidently been checked by insuperable difficulties; and certain formations appear to have had a greater tendency to assume certain external forms, suited to their internal constitution.

The crystalline granular siliceous rocks occurring as independent veins, that is, as veins which do not branch out from the large masses, viz. greenstone veins with their parallel sides, are certainly greatly opposed to our theory; and indeed that theory will seem to many at once to deserve rejection, when they remember the remarkable fact of gneiss fragments occurring in the above-mentioned veins. I grant that the difficulties are considerable; but even should this single case be quite insurmountable, yet still I do not find, on that account, that there is any sufficient ground for giving up the proposed views, and for returning to those other ideas, which, not merely one, but a large number of facts have proved to be faulty and unsatisfactory. The experience connected with the old theories ought to render us cautious respecting our expectations of the new one; all that we can hope is to bring an idea upon the path as somewhat more productive, somewhat more correct, than those with which we have hitherto endeavoured to assist ourselves. I must also remark, in regard to the proposed theory of the origin of the massive rocks, that it must always be kept in mind that it by no means pretends to be without exception available;—to which of these rocks it is to be applied is a problem which remains yet to be more exactly determined. When we were considering the crystalline masses occurring in Vesuvius, which, so far as we are informed by descriptions, have completely the forms of our greenstone veins, we did not doubt of their having resulted from erupted lava; we considered it further as extremely probable that a number of masses having other forms likewise came from the interior in a melted condition, and by slow consolidation and great pressure assumed even a granite-like nature. Thus far we have granted, and thus far we shall concede to volcanism all its just rights; and the views now developed only claim to be placed at the side of the other theory. More extended investigation will perhaps shew that, instead of being opposed to the volcanic system, these views, on the contrary, approach to a union with it. If metamorphoses take place, in general, in the way we have imagined, there is then no ground for assuming that merely original Neptunian, and not also volcanic rocks, were subjected to them. But a more intimate union of ideas of the pyrogenetic formation of the massive rocks, and ideas of their production by metamorphoses, may be imagined; so that both opinions may possibly be included under one and the same higher conception. One has merely to examine a little more attentively what takes place when a liquid mass becomes solidified,—for example, when it becomes granite. If we attempt to take the subject into consideration in somewhat more than a superficial manner, we shall find ourselves obliged to go into ideas not disagreeing with that theory to which our previous examinations have led us; and processes of transition of amorphous and homogeneous masses, into masses that have become crystalline and heterogeneous, perhaps involve the same principles. Whether the masses were compact, or pasty, or quite liquid, it is probably not too daring to hazard the conjecture, that the essential distinction in the processes will be found to consist entirely in the time required for the development of crystalline individuals. But respecting all this it is necessary to wait for the further progress of investigation. At present my object is to protest against the demand that our theory shall explain every thing, and against the notion that, if it should not account for the phenomena of greenstone veins, therefore it should be rejected in reference to all the other massive rocks.

It is, besides, by no means my opinion, that the veins which I have termed independent, those namely consisting of aphanite and greenstone, cannot really be regarded as metamorphic rocks in the same manner as granite, the great porphyry formation, and the eurite porphyry occurring in the form of beds. If it is true that the veins branching out from the great masses, owe their origin to the same changes which produced these masses themselves, then the independent veins may have been formed in the same manner; besides, the rocks belonging to the latter have perfect analogues in other masses occurring more or less in the form of beds. But direct proofs are not wanting of the formation of these veins by metamorphosis; at least I do not see how we are otherwise to explain a whole multitude of facts, which I have described in the work already mentioned, and that is about to be published;* but, for the present, I must content myself with thus referring to these facts, as the descriptions connected with them would lead to the consideration of phenomena which are partly of an extremely complicated nature. In the mean time, I must confess that, as the study of every thing relating to this subject is but newly commenced, we are not yet in possession of results with which we are to be satisfied. I do not, therefore, wish to attack any one's opinion regarding these veins; but I would remark that we ought to adhere to a theory which seems to be the only one applicable to at least the largest number, and the most important of the facts which I have now adduced.

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* The author here alludes to his "Gaea Norwegica," which has been published since the appearance of the present memoir, and of which a short account will be found in our last Number, p. 215.—Edit.

We have still to consider the alterations in the stratified rocks where these come in contact with other rocks,—namely, with the abnormal rocks; and to direct our attention to the peculiar minerals which present themselves either at or near the contact of the various masses which touch one another. This study of both kinds of phenomena is that which leads most directly to a knowledge of the forming and transforming processes going on in solid rocks, without the aid of an extraordinary degree of heat. That such processes have been especially active where heterogeneous masses met together, is to be observed at innumerable points in new and old formations; and among these, there are frequently places where it is impossible to maintain that volcanic heat could have operated in producing the phenomena in question. The relations presented by these phenomena in the Christiania district, are of extreme interest and importance; they point to a much more complicated play of forces than the mere action of a melted mass on the bounding rock could have effected or produced. These phenomena sometimes present themselves, and are sometimes absent, near the massive mountain-rocks; sometimes also they present themselves at the points of contact of *stratified rocks alone.* The examples of the last mentioned appearance presented by our district ought not to be passed over, although they only contribute indirectly to the explanation of the real subject of our investigation. Where transition-strata cover the primary surface, which, as already mentioned, consist of stratified rocks, there is constantly an extraordinary quantity of *silica* present, and *metallic developments* are observed at many points of contact. By this unusual quantity of silica, are produced the number of quartzose masses which lie at the boundary of the primary class, and with which the eurite-porphyry series begins; and to the same cause is undoubtedly to be attributed the frequent occurrence of certain quartz druses in the upper crust of the primary rocks, where the latter have been covered by transition-deposits. Of ores, we met with accumulations of magnetic iron-ore and copper-pyrites, which lie especially in the primary outer crust; but above all, iron-pyrites, which is distributed through the whole lower part of the transition series; it is on this account that alum-slate occurs here, instead of the usual clay-slate, inasmuch as the alum-slate is nothing else than a mere modification of the ordinary strata produced by contact with the primary rocks, and which has been indirectly or directly impregnated with iron-pyrites (and potash ?) by means of this contact.*

The actions which produced alterations in the strata in contact with the boundary of the granite masses, and the formation of the new mineral products occurring there, are of themselves of subordinate importance,† compared with the results of the alterations and formations which we have last mentioned; but nevertheless, the contact phenomena of the granite boundaries are the most self-evident. The conversion of fine lamellar soft clay-slate into a coarse lamellar siliceous slate, and that wonderful passage of limestone into a more or less perfectly white crystalline marble, are remarkable phenomena; but the extension of such actions to the distance of more than an English mile (1–6th of a Norwegian mile), from the granite boundary, is in the highest degree striking; so also is the occurrence at the same boundary of numerous masses of ore, of garnet, and other remarkable minerals. The silicification, and the tendency to crystallize, which are in general observable at these boundaries, shew that the actions here were so far perfectly analogous to that by which granite itself was formed; but the substances actually formed at the contact, viz. ores, seem to require the application of the idea that the new materials have been brought from a distance to certain points, where we find them collected,—a process that may still be going on, as the formation of these mineral masses seems so certainly to be dependent on the contact of rocks of a different nature, and whose contact moreover is permanent.

It is a remarkable fact, and one which has not yet been narrated, that the usual changes of transition-strata at their boundary with granite, have not taken place where the primary rock was near enough to cause the occurrence of alum-slate; and the latter is never found passing into hard slate, although quite near the granite. But there we find chiastolite, a remarkable proof that the strong tendency to crystallization nevertheless existed also at those places. We remarked of eurite-porphyry, which, for reasons now clear, occurs chiefly in alum-slate, that it has not the power of producing any alteration on that rock at the junction; thus affording a sign of a power acting against these metamorphoses, whether that power be in the condition of the alum-slate itself, or in the causes of the formation of the alum-slate (contact with the primary series).

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* Forchammer found potash in the alum-slate of the north.

† See the observations on alum-slate in the next page.

That otherwise, the porphyries in general do not produce these remarkable contact phenomena at the granite or at the primary boundaries, is a fact which deserves to be again mentioned. Melted masses, whether they afterwards become hardened into a porphyry or a granite, must, by their heat, have produced nearly the same actions. But if we assume that these formations of minerals, and changes at junctions are results of actions that required much more special conditions, we are no longer surprised to see them sometimes only weak, and at others entirely awanting in masses, where, according to the volcanic interpretation, they ought to have been developed in the highest degree.

But, for the present, we must be satisfied with giving the above details. Whoever, in reference to our very remarkable transition district, may wish to test more exactly the views now given, by means of a larger collection of facts than I have here noticed, will find sufficient data regarding highly instructive localities in the work to which I have more than once referred.

Before proceeding, in the next place, to pass on to the consideration of the massive formations of other countries, which ought to be placed alongside of those occurring with the fossiliferous transition series of Norway, I would add, from my still unpublished work, one general reflection in regard to the chief idea in the proposed theory,—the idea, namely, that crystals, and even whole aggregates of these chemically and morphologically most perfectly formed inorganic natural bodies, can proceed from an indeterminately compound, formless, material, by means of slowly operating actions in the solid masses: my observation on this point is the following,—that it would be a much more beautiful result of such investigations, if we could believe that even the mass of the earth is at all times in a condition to advance onwards to a state of greater perfection, and that individual development from the *chaotic* may likewise proceed gradually, instead of our regarding the body of the earth as a *cadaver* in which all processes merely tend to a retrograde movement, to chemical decomposition and mechanical ruin.

Observations on the Virilescence, Feminescence, and Rejuvenescence of Animals. By Dr Mehliss.*

In animal bodies there are three distinct periods or epochs,—viz. of formation, of growth, and of decay. The less perfect animals cease to exist as soon as they have attained their full growth, and become capable of propagating their species: the act which marks the perfection of their development is but the prelude to their destruction. In the higher animals, however, a longer or shorter period elapses after they are no longer capable of procreating their species; a period of gradual decay and progressive exhaustion of all the powers of life. This period is longer in man than in any other animal, and presents some phenomena which are well deserving of attention.

Dr Mehliss of Liebenwerda, in his curious work,† has chosen two of these as the subjects of discussion: the assumption of the characters of the male by aged females, and the reappearance of the characteristics of youth in the aged of both sexes; to the former he gives the name of *Virilescence,* to the latter that of *Rejuvenescence.* He might likewise have added another new term, indicative of the appearance of some of the virile characters in the female, as he has noticed this circumstance in his book; and he might have named this condition *Feminescence*, from the same analogy on which the other terms are framed.

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* British and Foreign Medical Review, No. xi. for July 1838.

† Ueber Virilescenz und Rejuvenescenz thierischer Körper. Ein Beitrag zur Lehre von der regelwidrigen Metamorphose organischen Körper. Von Dr Carl Wilhelm Mehliss, praktischen Artze, &c. in Liebenwerda.—Leipzig, 1838.

On the subject of Virilescence, the author quotes the authority of Hippocrates and Aristotle, and traces the origin of the fables of the middle ages concerning a "mutatio sexus" to the facts which they have related, and to the mythological sayings of the Grecian poets; yet such a change was not entirely without support from ascertained facts: old women had been seen with beards, and their voices had assumed the manly character; persons who had been educated as females had been seen to assume all the attributes of men; and others, who as women had remained barren in marriage, became the fathers of children. Such instances as these, which a careful inquiry would have robbed of all their value as facts, were strengthened by the opinion of Galen that the parts of generation are the same in both sexes, and vary only in position, the same parts being situated externally in men which are placed inwardly in females; by that of Aristotle, that a woman is merely an imperfect man; and by the scholastic dogma, that Nature, under all circumstances, aims at perfection. Eusebius Nieremberg and Ulysses Aldrovandus had done their best to confirm these errors, by tales of exotic animals in which this conversion of sexes was a natural characteristic; and the doctrine was not entirely abandoned, even by anatomists, before the end of the sixteenth century. The seventeenth century, however, saw the error completely dispelled; and since then the whole subject of mutation of sex, instead of being a tissue of fables, has become an interesting branch of natural history.

Virilescence.—Those appearances which our author includes under the general name of virilescence, occur in females in whom one or more of the peculiar characteristics of their own sex have passed away, and consist in the assumption of some peculiar qualities of the male. The assumed characters differ in different cases, but agree in bearing a close resemblance to some part of the male frame, other than the organs of generation themselves. Virilescence, therefore, can take place only in those animals which continue to live some time after the power of generation has been lost, and in which the sexual differences are sufficiently strongly marked, not merely in the organs of generation themselves, but in other parts of the frame. Consequently, it cannot occur in vegetables; for the difference of sex is here only marked in the sexual organs themselves. Kob, indeed, has compared the regular metamorphoses of the organs of generation of plants into other parts (as, for instance, of styles into stamens, and the occurrence of male blossoms on the female plants of the class Diœcia), with the appearance of virilescence in animals; but these deviations from the normal condition are original, and do not admit of comparison with the animal phenomena, which distinctly require the complete development of the sexual character at a former period of life. This condition is fulfilled only in the more perfect animals—in birds, in the mammalia, and in man. In insects, the sexual difference is sufficiently clearly marked in the external conformation of their bodies; but their life, closing with the process of generation, is too short to exhibit the phenomena of virilescence. In some of the Crustacea, in fish, and in amphibia, observation will probably yet detect the existence of these changes. In all animals, however, and under all circumstances, virilescence is extremely rare.

In birds, the chief distinction between the sexes, after the differences between the parts of generation, consists in the plumage, which is more developed, and possesses more lively and varied colours, in the male than in the female. The size of the body, the character of the voice, and the occurrence of spurs on the feet, establish other, but less striking, distinctive marks. In each of these particulars, the female bird may simulate the outward appearance of the male, and to such an extent that a careful observation shall scarcely distinguish the sexes. Our author collects abundant proof of this fact from undoubted authorities: of these, the greater part occurred in the domestic fowl and common pheasant; others in other species of pheasant, in the turkey, peacock, and common duck. The partridge, wood-pigeon, starling, bustard, chaffinch, and four or five other birds, have been observed to assume the same peculiarities. The change of plumage was the most remarkable, as well as by far the most frequent, phenomenon; the alteration of the voice was nearly as common; the growth of spurs, combs, &c. is reported in more than one case. These changes began after the bird had ceased to lay eggs, and became more marked as it grew older. In some instances the transformation was so complete, that not only was it difficult to make a distinction between the real and the assumed sex, but the difficulty was scarcely less for birds of the same species; especially as the deception was increased by the masculine behaviour of the transformed birds. In a few cases, eggs were detected in the ovary; and, in a large proportion of instances, the parts of generation were found partially or entirely wasted away.

In mammalia, the signs of virilescence are neither of so frequent occurrence nor so strongly marked as in birds; and hitherto they have been seen only in the stag and roe. The male of these animals, as is well known, is distinguished by horns, of which there is no trace in the female. These parts make their first appearance with the first complete development of the organs of generation; fall off annually, at the conclusion of one rutting season; and are again reproduced, in a more perfect form, at the beginning of the next. Virilescence consists chiefly in the growth of these parts in the female: it shews itself, however, occasionally in other parts; for instance, in the hair. More than one of these changes have been seen in the same animal. In some instances both horns were produced; in others only one, and that usually on the right side. Two instances only are adduced in which the phenomena of virilescence showed themselves in the hair of the animal. In one case, the hair of the head, neck, and abdomen, the shape of the ears and extremities, and the odour of the animal, gave it the closest possible resemblance to the male, and it followed the other females as if urged by sexual desire. Valmont de Bomare has described an animal which had one horn on the left side, and organs of generation closely resembling those of the male; the ovaries hung down like testicles, the clitoris was elongated, and the vagina contracted. In three instances, the animals were in calf. The case quoted from Valmont de Bomare is the only one in which the parts of generation underwent those changes which take place so frequently in birds. The statement that horns have made their appearance on the heads of animals which do not usually bear them,—as, for instance, the hare,—is justly treated as a fable; and the growth of horny excrescences on the skin of men and animals, is shewn to have no connexion with the phenomena of virilescence. The change of the colour of the skin, too, which is observed to take place in old animals of both sexes, is shewn not to be allied to virilescence.

In the human species, the phenomena of virilescence consist in the growth of hair, partly on the face, in the form of a beard, and partly in other situations, where, in ordinary circumstances, it does not exist to any extent, even in men; and in a strengthening of the tone of the voice, so as to resemble that of the male. The facts which are quoted by our author are not very numerous, amounting only to eight detailed cases, and a few references to others. With the exception of the case of extirpation of the ovaries, related by Pott, the phenomena of virilescence are referrible to suppression of the menstrual discharge, occurring as well at an early period of life as in more advanced age. The growth of hairs on the upper lip in young persons, a short time before the appearance of the menstrual discharge, is regarded by the author as an event of not unfrequent occurrence.

The following general conclusions are drawn by Mehliss from the facts adduced by him :

1. The changes which take place in animal bodies during virilescence are either organic or dynamic. To the latter belong the changes of the voice and of the sexual characters. The former are of two kinds: in some cases a degeneration of structure occurs, as the gradual wasting away of the sexual organs; in other instances there is an actual development of parts which, in the natural state, exist only in males. The new growths which thus take place, either already existed in a rudimentary form, and are merely more fully developed (for example, the comb in the common fowl, hair in women, crista pectoralis in the turkey); or they already existed in the female under a different form, and merely undergo a change (for instance, the feathers of birds); or, lastly, they were originally absent, and must be considered as entirely new formations (as horns in the hind, spurs in the common fowl).

2. The changes take place gradually, obey the laws to which the development of the same parts in the male is subject, and follow the same order.

3. In all the individuals which have exhibited the phenomena of virilescence, the characters of their own sex were already fully developed; the greater number, too, had been fruitful; and it is only in some individuals of the human species that the power of reproduction has been wanting.

4. The appearance of virilescence was, in all cases, attended either by an entire loss, or a remarkable diminution of the power of reproduction; and, as the virilescence increased, the power of reproduction diminished, at length entirely disappeared, and never returned.

5. On the other hand, virilescence seems to have commenced, in most instances, before the usual period of the disappearance of the sexual functions; so that it did not begin with the commencement of *old* age, but a longer or shorter time before it. In men, and in the mammalia, it occurs proportionably earlier than in birds.

6. All the individuals in whom virilescence has been observed, have been placed under favourable circumstances. They all appear to have been remarkably robust, powerful, and healthy, and to have reached a good old age.

7. In no case was the health of the individual impaired by virilescence; nor was the occurrence of the phenomena accompanied by feelings or symptoms of sickness.

Feminescence.—After closing his account of Virilescence, the author notices more briefly the reverse condition, or the assumption of some of the male characters by the female, which we might term *Feminescence.* The two principal circumstances noticed under this head are, the appearance of the catamenia, or, at least, of a sanguineous discharge in the male; and the secretion of milk by the male breast. Our author quotes the authority of the ancients for the occurrence of the first-named phenomenon; but not a single credible instance, as might be believed, is adduced. The most curious statement under this head is the assertion by certain authors that the catamenial lustration from the penis was inflicted on the *Jews* as a divine punishment! On the other hand, the secretion of milk in the breast of males is a fact sufficiently attested, both by ancient and modern writers. Here, also, credulity has added her legends. In the sixteenth century, some missionaries in Brazil asserted that there was a whole Indian nation whose women had small and withered breasts, and whose children owed their nourishment entirely to the males! It is, however, true, that one of the best authenticated instances of this fact occurred in a South American, and is related by Humboldt. Francisco Lozano, aged thirty-two, a peasant of a small village in Cumana, nourished his child with his own milk. His wife, immediately after her delivery, fell sick; and Lozano, in the hope of quieting the child, applied it to his breast. A secretion of milk took place, which gradually increased until it afforded sufficient nourishment for the infant. Humboldt and Bonpland were assured by eye-witnesses, that during five months the child took no other nourishment whatever. Humboldt saw both the father and son; and states that the breasts of the former closely resembled those of a female. We may add the following very similar fact, on the authority of one of our most enterprising travellers. A young Chipewyan lost his wife in her first pregnancy: he applied the infant to his breast, to still its cries; and "the force of the powerful passion by which he was actuated, produced the same effect in his case as it has done in some others which are recorded: a flow of milk actually took place from his breast." "Our informant, Mr Wenzel, added that he had often seen this Indian in his old age, and that his left breast even then retained the unusual size it had acquired in his occupation of nurse."* Some remarkable examples also are given of male animals who had given suck either to the young of their own or of other species, or had furnished milk to man. The occurrence of this abnormal secretion was in no case accompanied by a change in the functions or structure of the parts of generation.

We cannot follow our author in his examination of the somewhat analogous, but really very different, subject of *Hermaphrodism;* neither can we give his discussion on the causes of the singular change to Virilescence. We give the conclusion to which he arrives in his own words. "Virilescence," says Dr Mehliss, "like many other degenerations to which organic bodies are liable, is a means of compensating a disproportion which exists in the organism between the energy of the vegetative life of the whole body and that of individual organs."

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* Franklin's Voyages, vol. ii. p. 54.—*Richardson's Journal.*

Rejuvenescence.—The idea of the assumption by aged persons of the characters of youth was familiar to the ancients, and probably formed the groundwork of the fable of Medea and Æson. Pliny and his successors have related instances of the kind, and modern writers have added to our stock of facts. No one, however, we believe, before Dr Mehliss, has attempted to collect the scattered cases to ascertain their credibility, and thus to make the whole subject a branch of physiology. He arranges the phenomena of rejuvenescence under five distinct heads:—1. The secretion of milk by aged females. 2. The return of the menstrual discharge. 3. The cutting of teeth in old age. 4. The growth of hair similar in colour to that of the young. 5. The sharpening of the intellect, and restoration of the vivacity of youth to the old.

It would occupy too much space were we to notice, however briefly, the various facts which are here detailed: and we must refer our readers to the original work. The subjoined table shews, at one view, the number and nature of the principal facts.

	Number of instances of			
	Dentition of the aged.		Lactation by aged females.	Menstruation of aged females.
Age of the individuals.	Males.	Females.		
Between 40 and 50	0	4	2	1
... 50 ... 60	1	4	1	0
... 60 ... 70	3	2	0	7
... 70 ... 80	3	2	0	0
... 80 ... 90	6	2	0	0
... 90 ... 100	1	1	0	1
Above 100	6	1	0	1
Total,	20	16	3	10

It is stated by Dr Mehliss to be a necessary condition for the appearance of the phenomena of rejuvenescence, that there exists complete energy and integrity of vegetative life at the period of decrepitude. It is also shewn that the mode in which this energy shews itself depends upon local causes. The secretion of milk, the return of the menses, and the growth of the teeth, may in most cases be attributed to an irritation applied to the parts concerned. In some instances, these changes are accompanied by symptoms of constitutional irritation, and fatal results have followed in more than one case; a fact which connects the physiology of the subject with practical medicine.

Dr Mehliss's book is, on the whole, interesting; and contains many curious details, and some important physiological views.

Description of an Improvement in the Common Vice, and Vice-Chuck, whereby the action of the Screw is made perfect.[*] By Mr Robert Wilson, Engineer, Paul's Work, Edinburgh. Communicated by the Society for the Encouragement of the Useful Arts in Scotland.[†]

In the common vice-chuck and hand-vice, the screw is fixed to one of the arms, and the nut which works on it revolves; and as the arms are jointed, it follows that to render the application of the nut alike for all distances to which the cheeks may be opened, the screw must be bent, or some other contrivance must be had.

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[*] Read before the Society for the Encouragement of the Useful Arts in Scotland, 15th Feb. 1837; and obtained the Society's Silver Medal, value Five Sovereigns, 6th December 1837.

[†] *Report of the Committee of the Society of Arts for Scotland on Mr Wilson's Improvement on the Common Vice and Vice-Chuck.*

The great and acknowledged defect of all vices which turn on centres is, that the screw cannot act fairly at all distances. This defect is found also in spring-compasses and callipers.

The very simple and efficient plan adopted by Mr Wilson is to give to the moveable arm such a form as may cause the nut to bear upon its diameter, to whatever distance the cheeks of the vice may be opened; in this way preventing entirely every undue strain upon the screw.

Your Committee have great pleasure in reporting, that the curve given by Mr Wilson completely produces the required effect; and that there is this farther advantage in the method, that it can be applied even to vices that are already constructed,—a piece of steel of the requisite form has only to be placed between the washer of the nut and its present bearing.

When the screw is bent, the separation between the threads on the convex side is greater than that on the concave side of the screw,—so that no nut can passably revolve upon such a screw, and apply properly to the threads at the same time. The line of the strain is the straight line which joins the two ends of the bent screw, and thus there is a tendency to straighten the screw or change its form. In every point of view the bent screw is bad.

In the bench-vice, again, the screw revolves in the nut. The nut-box is generally long, so that the screw cannot be formed in a curve; hence, to allow for the angular motion, the nut must be suffered to work loosely on one arm, while the shoulders

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There is also this further advantage, that a turn of the screw produces always the same angular motion in the arms; a circumstance which is peculiarly recommendatory in jointed compasses and callipers.

Considering the frequent and essential application of vices to the useful arts, your Committee most earnestly recommend the form proposed by Mr Wilson to the notice and adoption of the members of the Society. They beg also to recommend, that a notice of this improvement should have a prominent place in the Society's Transactions, in order to its adoption in the manufactories of the South. A very slight deviation from the common forms of the tables and bench-vices will, without the slightest additional cost, give them prodigious advantages.

The case where the screw is firmly fixed in one arm of the vice, is completely solved by Mr Wilson; but that in which the screw is jointed on the fixed arm, does not seem so satisfactorily made out. The investigation of the true curve with that arrangement would be rather intricate; and it is a question if, seeing the perfect action of the fixed screw or screw-box, it be worth while to joint it.

In the case, again, of spring-callipers and compasses, a peculiar curve is needed; the form of which depends on the form and flexure of the spring. The investigation of this form is beyond the power of the present methods of analysis, as it necessarily requires the subsidiary investigation into the form of the spring itself, an investigation which has not as yet been accomplished. Still the principle of Mr Wilson's contrivance is applicable to these cases; and the forms can be discovered with sufficient nearness for practical purposes.

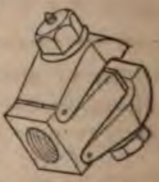
Edinburgh, 21*st June* 1837.

of the nut and screw cannot bear always in the same way on either side. Thus the strain, instead of being transferred along the axis of the screw, is sometimes transferred from the upper, sometimes from the under edges of the bearings, and, but rarely, from face to face.

In both the bench and hand-vice, then, the flexibility of the screw allows the oblique pressure to bend it, and the articles are held in the vice by this elasticity; and this is one reason, that although the cheeks of the vice have met upon a hard object, as a piece of iron, the screw can be turned considerably farther.

But the elastic or insecure holding is not the only fault of the common vice,—the parts obliquely strained wear out very soon!

To remedy these evils in the chuck and hand-vice, I fix a straight screw on one of the arms as firmly as possible, and work it by means of a nut accurately fitted; or in the table or bench-vice, I fix the box, instead of the screw, to the stationary arm, and work it by means of a straight screw,—or allow the nut to be loose, as at present, which gives it the same advantages as the screw.



The next thing is to cause the nut, or shoulder of the screw, to work square upon its bearings, so as to transmit the pressure directly along the axis of the screw. This I accomplish by making an elongated hole in the moveable arm for the screw to pass through, and by forming the sides of that hole to a particular curve. The nature of the curve is this:—in any position of the vice, if the plane where the axis of the screw cut the curve be noticed, a plane touching the curved surface must be perpendicular to the axis of the screw; by this means, as will be seen on inspecting the instrument, the screw and nut always act directly, giving a full, steady, and dead pressure to the article grasped,—while all the parts are saved from undue wearing. The peculiar form of the curve gives it other advantages, especially it increases the strength.

Edinburgh, 19*th January* 1837.

On the Lunar Mountains Tycho, Pictet, Saussure; and on the Geology of the Moon. By William Beer and J. H. Mädler of Berlin.[*]

1. *Topography of the Surface of the Moon.*—The lunar topography of Messrs Beer and Mädler occupies 213 quarto pages, and is divided into four sections, corresponding to the four sheets of their map, or to the four quadrants of the circular surface of the moon which is visible from the earth. The authors give successively, in each section, detailed descriptions of the seas, and of the different regions which correspond to them, also of the mountains, cavities, and all the other appearances which they present. Hence, it will readily be understood, that it is quite impossible to present any thing like an analysis, properly so called. We shall, therefore, confine ourselves, in presenting an idea of the method pursued by our authors, to the selection of one of their descriptions, viz. to that of the large spot Tycho, and of its environs, which will be found at the commencement of their third section, which relates to the south-east quadrant of the moon. And even in this, we must omit certain less important details, and some which have already been alluded to.

Tycho, so named by Riccioli, and which Hevelius had designed *Mons Sinai* and *Desertum Zin*, is an annular mountain, which is visible to the naked eye during full moon; whilst, at the epoch of its changes, it cannot certainly be distinguished except by means of a good glass, and with a thorough knowledge of its precise position. It is situated, according to our nine measurements, in 42° 52′ 19″ of the moon's southern latitude, and in 11° 52′ 25″ eastern longitude; it is 11¾ German[†] miles in diameter (about 20 leagues of 25 to a degree); it is of a round form, and is completely surrounded by a narrow rampart of a uniform height, very like a white wall. Our measurements give to its eastern edge an elevation of 2676 toises above the interior surface, and a height of 1961 toises above the terraces lying in front of it; they also supply an elevation of 780 toises for the central mountain, and 2509 toises for the eastern wall. The central mountain, then, should be nearly of the same level with the terraces. These terraces surround, in a series of from three to five ranges, the internal foot of the zone, always leaving a considerable portion of the plain free; and in which, besides the central mountain, some hills may also be observed. As, however, it is only two days after the rising of the sun, that the plain is so free from shadow that this can be distinguished from the base of the central mountain, and as at this epoch the lower hills have long become wholly invisible, it is not certain that the interior is quite so plane as is represented.[*]

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[*] We again avail ourselves of the able abstract of the labours of Beer and Mädler, prepared by M. Gautier of Geneva, for the Bibliotheque Universelle.—Edit.

[†] We must again remind our readers, that the miles in this article are *German* miles, each being equal to about 4.6 English. The measurements of heights are given in *toises*, a *toise* being equal to 6.39 English feet, or, for general purposes, a fathom.

All around Tycho are to be seen hundreds of peaks, and ridges of mountains and craters, so that it is impossible to find the smallest level space, and it requires a long series of the most careful observations to determine successively all the details which are required in designing the map. In the midst of this kind of chaos, which is so irregular in appearance, we may observe that the eastern and south-eastern craters, and the long range of mountains at the west, whose direction is parallel to that of the margin, form the principal traits of the picture. The mountain chains on the north are grouped together, without being parallel to the margin of Tycho; and it is quite impossible to disentangle the greater number of those to the south. At a distance of from four to six miles, the large craters and the annular mountains begin to reappear; only a small proportion of them are quite circular, although all of them approximate to this figure. Very elevated zones surround and traverse them, so as to interfere with every thing like regularity.

At no great distance to the west of Tycho, we see the annular mountain *Pictet*, which is a very irregular hollow, of between 400 and 500 toises, surrounded by a zone, which is formed of ridges of mountains, high and low, and of craters. The most considerable of these is situated at C, in south latitude 41° 33′, and in east longitude 8°. Another annular mountain, situated to the south, and to which Messrs Beer and Mädler have given the name of *Pictet* α, is smaller, but less irregular and more visible, and is very completely seen at full moon. Its zone is considerably elevated towards the north-west; at some points it is interrupted, but, so far as can be judged, it is without craters. At full moon, five peaks can be discovered in the zone of Pictet, like brilliant points, which are scarcely detached from the great luminous band which runs in that direction.

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[*] It may be curious to compare with these dimensions of Tycho, those of the spot Aristarchus (Mons Porphyrites, H.), so remarkable for its lustre. Aristarchus is nearly a circular mountain of six German miles (ten leagues) diameter, which has a central mountain in its most brilliant point. Its western edge is elevated 1176 toises above the cavity, and 414 toises above its external base. The eastern part of its zone connects Aristarchus with Herodotus, likewise an annular mountain, and upwards of eight leagues in diameter; it is steeper, but not so deep, and has no central mountain. There is in the part of its zone near Aristarchus two summits, α and β, elevated about 686 toises above the cavity; this last is very obscure, although it is about 300 toises higher than that of Aristarchus, which has a brightness of 10° over its whole extent. The margins of Aristarchus have a brightness of from 9° to 6°; the peak α of Herodotus has 7°, and β scarcely 3°. These two mountains are situated to the north-east of the moon's centre, in the most sombre portion of the *oceanus procellarum*, and have scarcely any connection with other mountains or bright regions.

Although it is difficult to take measurements in those situations where the limit of the light is quite undetermined, yet we have made some attempts of this kind, which we supply.

Tycho A, western margin, has a height above the bottom of the cavity of	835 T.
Tycho D, western margin,	817
Street, south-western margin,	698
Pictet α, western margin,	889

From these measurements, it appears that the most marked differences in elevation in the immediate neighbourhood of Tycho reach to about one-third of those which exist between its principal zone and the bottom of its own cavity.*

Saussure is a great annular mountain, which had not previously been named, and which is situated somewhat to the west of Pictet: its form upon the whole is regular; it is six German miles in diameter, and is visible at full moon, because it is somewhat darker than the surrounding region. The luminous bands which extend from Tycho to its eastern border are there interrupted, and then continue in the same direction as far as its western margin, which is the only example of this kind of appearance found in an annular mountain of such extent. Its interior, whose brightness is at 3°, is flat, and exhibits only one feeble crater, placed excentrically. Its zone is highest at the points β and α: a terrace may be seen here, which appears to cease towards the western margin. The most luminous of the craters which interrupt the zone is marked by the letter B, and is situated in 42° 25′ south latitude, and 3° 36′ west longitude; four small summits crown the steep zone. The luminous bands which are situated near the edges of Saussure have the same degree of brightness, with somewhat clearer places towards the south. On the exterior flank, towards β, a small furrow or cleft (Rille) commences, running towards the north, which was discovered on the 18th of March 1834. Two other somewhat analogous valleys, which run south near γ, had previously been remarked on the 29th of March 1833. On the west from the point ε, proceeding towards the north, there exists a great mountainous ramification, parallel to the western margin, and more elevated, which connects itself with the mountains of Orontius. The annular mountain we have designated Saussure *A*, situated to the west, in 43° 20′ latitude, and 1° 14′ longitude, is very regular, and in a somewhat flat district. Beyond it, more to the south, is found the double

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[*] It will here be remembered, that for most of the smaller heights, and for a considerable number of the mountains of the moon, Messrs Baer and Mädler have not taken actual measurements, or actually calculated the elevations; but have limited themselves to a simple estimate of these heights, the result of a visual appreciation of the length of the shadows, compared with those of the neighbouring mountains, whose heights had been accurately ascertained with all becoming care.

crater C, whose cavities are connected among themselves: still smaller ones are in the neighbourhood, and somewhat further off is the crater *b*, of a regular and very distinct form. We may say that this region *swarms* with craters, some of the diameters of which do not appear to exceed half a second, and which are notwithstanding upon the whole more visible than the intervening mountains: these are seldom 300 feet high, and are chiefly distinguished by the total absence of more elevated mountains.*

It cannot be doubted that Tycho is the point of departure of a system of rays, which occupies in its whole extent at least a quarter of the visible surface of the moon. * * * The bands are so approximated towards the west, that they unite in an almost continued surface over the south-west quadrant, so that it is difficult to perceive any difference; and the dazzling brightness in no slight degree increases the difficulty of making investigations respecting this region. They are lost towards the margin. These bands become visible as soon as the sun rises from 20° to 25° above the horizon, whether Tycho itself is illuminated or not, and they disappear when the sun redescends from them to the same extent. A portion of the more luminous, especially those which are situated upon a dark basis, are still for a long time visible, but none at the rising and setting of the sun. At the same time some may be distinguished in the obscure part of the moon by the simple effect of the illumination from the earth.

It is only during the full moon that we can see the radiated system completely, and it is best distinguished when the latitude of the moon is northwards. So soon as the shadows of the mountains begin to appear in any region, the bands disappear, and conversely. To such an extent is this true, that so soon as the system of bands appear, you can no longer detect the slightest traces of the larger and highest annular mountains, or even mountain-chains, even when you are intimately familiar with their exact position. Still, however, very feeble traces of some of them may be discovered, such as Saussure, Piccolomini, and Lindenau, as well as Mount Altaï, and some other neighbouring objects; but for this first-rate instruments, and a very favourable state of the atmosphere are required. There are also some small craters (small in number, and generally not remarkable), which are visible as luminous points in the interior of the bands; the steepness of the slope, more or less, has no influence on this matter; and there is in fact no *criterion* of the visibility of these objects to be discovered when the moon is at the full.*

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* Though the mountain which is called *Deluc* is somewhat more distant from Tycho, to the south of Saussure, it may be interesting to supply a short description. It is an annular mountain, of a circular form, with a diameter of *six German miles*, enclosing a small central mountain. Its western margin, which is the most elevated, does not exhibit a distinct peak, as does the eastern, which is broader withal. Two still smaller annular mountains, which are close together, *d* and H (the latter situated in 54° 8′ south latitude, and 2° 32′ east longitude), are symmetrically situated to the south and north, and the latter has a central mountain. The point *k*, situated in the neighbourhood, is the only surface which appears obscure at the time of full moon, and consequently remains visible even then, when its locality is well known; the point *m* also remains visible like a faint ring of light: every thing else disappears. This region is to be found near the first meridian of the moon, passing from the centre to the poles. The crater Deluc E is situated towards the south-east limit of this region, in 66° 20′ latitude, and 5° 31′ longitude. The whole of this region exhibits few traces of past commotions, and nevertheless presents a very considerable luminous brightness.

About this period, the central mountain of Tycho is a clearly determined luminous point, of 8° of brightness. The whole circumference exhibits a low flat; a part of the lower terraces which surround it in two places near the eastern zone are at 5° of brightness, and the higher terraces are at 3° only.† The grand principal zone, about half a German mile broad, is at 8°, and this is the only one of this extent, to a distance of at least fifty miles, which shews itself by its brightness; and it is this circumstance which confers on Tycho its very striking appearance during the full moon.

A grey zone of 3° of brightness towards the outer base of the inclosure, and somewhat more luminous beyond, predominates all round this zone, to a distance of about five German miles, and it is this space like a dark crown, which Hevelius on his map denominated *Desertum Zin.* To its W.SW., at the distance of two miles from its margin, a small luminous point is perceptible, which is probably the point B; all the rest is a smooth surface.

It is from this grey zone, as it becomes more luminous, that there successively rise the bands which have been mentioned above, some proceeding from it directly, as that which runs towards Bulliald, whilst the greater number at first are confounded in a luminous areola, which, in some places, extends to a breadth of twenty German miles. As soon as the bands appear distinctly separated, we perceive between them, and even upon them, the luminous points in small numbers, of which we have already spoken. The majority of the craters have a brightness of from 6° to 7°, and the bands themselves from 5° to 7°.

In the north, to an extent of sixty German miles, only four distinct spots are to be found, and this not without much difficulty. Near one of them a whitish luminous spot may be found, at 9° brightness, which appears to proceed from the concourse of numerous bands. It is situated, so far as we have been able to judge, in quite a flat region, in 33° lat. and 3¾° long. This white spot is nothing less than the *whitish cloud* which was perceived by Cassini in the year 1671, which he proclaimed as a great discovery, and in place of which he saw in 1673 a new large spot. At the present day, an attentive observer may see a whitish cloud appear four days before full moon: he may witness it during the full, in all its brightness, and see some traces of it five days afterwards, without being able to discover spots of any other kind, for example annular mountains. But if he examine this same region, when it is near the limit of the light, the state in which Cassini found it in 1673, he will then very clearly distinguish mountains of this kind, which will be *new* to him if he has examined the region only by a more direct illumination. We can readily perceive to how many errors of a similar kind this change of aspect may lead; and it is owing to analogous optical illusions, proceeding from differences of illumination, or from variations in our atmosphere, that Messrs Baer and Mädler attribute, among other causes, the physical changes which Schroeter has thought he observed upon the surface of the moon.

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* Dr Mädler has given, in *Astr. Nachr.* No 283, by means of a copperplate impression, two representations of the region Tycho, on a single sheet, and on a scale double the size of the general map. In the one of these, the district is viewed in an oblique light; in the other as seen at full moon.

† This singular circumstance, of the terraces being more obscure than the cavity, appears to have led some observers to the conclusion, that the interior of Tycho is convex. But internal surfaces, which are truly convex in annular mountains, such as Mersenius, Petavius, and Hevelius, manifest their shape in quite a different way than by a greater degree of brightness; and it is always impossible to conclude directly the height of any point of the moon from the intensity of its light.

There is not a single one of the bands of Tycho which, according to our observations, shews the least elevation of level which is properly its own. It is thus that these bands present exactly the same intensity of light, whether in the plain of Stoefler, which is uniform like a mirror, or on the annular mountain of the same name, which is 2000 toises in height.

2. *Geology of the Moon.*—After having presented, in the foregoing quotations, a specimen, as it were, of the lunar topography of Messrs Baer and Mädler, we may now mention the manner in which they endeavour to account, in accordance with the known laws of physics, for the appearances which the surface of our satellite presents. However hypothetical these ideas may be, yet they possess a true interest as proceeding from observers so accurate and judicious, and they are likely to suggest new researches. They offer them with all possible and most commendable reserve. We take them from the concluding part of their treatise.

The authors adopt the idea of Count Laplace concerning the formation of the celestial bodies of our system. They admit that the moon was originally in a state of heat and fluidity analogous to that in which every thing seems to prove that the earth has been. They even suppose that the moon at first was in a gaseous state, and that it gradually passed into a solid condition, like the other planets and satellites, by a gradual condensation and cooling. This cooling, they remark, must necessarily have taken place sooner in the external parts than in the internal, and consequently a *crust* must have been formed when the interior was yet in a state of gas. Those portions of the mass which remained in this last condition in separating themselves from the molecules which were condensed, not being able to escape outwardly without opposition, violent ruptures and eruptions (*ausbrüche*) were the consequence.

We cannot assign a value to the time during which these reactions occurred, nor can we calculate their comparative force, or determine, *a priori*, what must have been the result in each celestial body in particular. The power of contraction in the several masses, the elasticity of the gases, the relation of the spaces to the different epochs of formation, the temperatures, and, finally, the weight, might and must present differences so great, that one body might experience only a variety of fractures, another so many elevations, and a third, over a certain extent, might experience neither the one nor the other of these effects. At the same time it appears, that in a small body the refrigeration of the surface should, in general, be more rapid, so that the internal spaces which remain free should, in proportion to the agitating forces, be more compressed than in a larger body.

It is perhaps on this account that the earth presents, in comparison of the moon, so few traces of these eruptions. The form of our globe generally is not characterized by them, but by upheavings and precipitations (*hebungen und niederschläge*). These last, again, seem to be entirely wanting in the moon. As to upheavings, they appear, at least in great part, to have given place in our satellite to complete eruptions, the result of which must have been the more energetic, since the space travelled over on the moon, as the result of equal eruptive forces, would be 6½ times greater than upon the earth. These effects have occurred neither at the same time, nor in the same external circumstances. The annular mountains, which exhibit the radiated system, would appear to have been the result of the most ancient reactions. The more recent encountering a harder substance had less powerful effects, and the dimensions of the crater ought, therefore, sensibly to be less; the eruption had both a more determinate tract, and occurred at a lower temperature. Besides, all the eruptions were not completely centrical; there were some which acted linearly under the surface, as the compressed ranges of annular mountains and craters prove, and not less the furrows (*rillen*), of which a considerable number are found upon the moon, although, in general, they are perceived only with difficulty.*

It would appear that there have also been some subsidences (*einsturzüngen*) at the surface of the moon, nor could it well be otherwise after such great changes. The round cavities without an annular mountain, such as are found in the region of Gauricus, as well as the great transverse fissures (*querklüfte*) near to Rheita, and in other parts of the south-west quadrant, as also perhaps the valley of the Alps, may belong to formations of this kind at the surface of the moon, in which eruptions may have had only an indirect effect.

The origin of the central mountains is very easily explained when they are considered as subsequent formations. The surface of the moon having been violently dislocated (*aufge lockert*) at the places where the first great eruptions occurred, they remained more liable to new ones; and when the endeavours at eruption were repeated, always somewhat more feebly, the actual ruptures took place principally upon the point of least resistance, that is to say in the centre of the annular mountains, and in that spot they elevated a mountain, or formed a new crater, sometimes uplifting also the whole interior, in the form of a protuberance or a bladder.

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* According to this hypothesis, Tycho would belong, in the opinion of Messrs Baer and Mädler, to one of the earlier formations, and its origin would reach to the time when the surrounding mountains, great and small, had as yet no existence. From this opening, the authors add, as from a point of general eruption, would escape from the interior of the moon, the elastic fluids which had been separated since its formation, and which were probably at a very high temperature. In thus acting under the surface they would change its internal structure, and by a process which we cannot now explain, they acquired this faculty of reflecting light; which operation, if named *vitrification* or *oxidation*, we will not gainsay, provided no ulterior consequences are deduced from the nomenclature. Perhaps the annular mountains and the craters which we still find in the radiated system, were formed in some places at the same epoch. But in the immediate vicinity of Tycho the effect was not the same, whether that the heat had previously diminished, or that the immediate neighbourhood of the opening enfeebled the energy of the action in some other way.

These eruptions, without doubt, present, in their general effects, some analogy with terrestrial volcanoes; but this does not authorize us to give them this designation, and thereby tacitly or expressly to assign them the same particular constitution. We can form no distinct conception of an igneous eruption where an atmosphere and water are wanting. The moon really exhibits herself as a very peaceable companion of the earth, and to say the least, there is no observation which obliges us to admit the contrary. Igneous eruptions properly so called, which can escape the observation of our glasses, directed to the obscure side of the moon, must be extremely small, and such as leave no durable traces such as we can discover. Shooting stars and meteoric stones, which Benzenberg attributes to the moon, are much more likely to proceed from celestial spaces than from the interior of our satellite, if they be not rather a product of our own atmosphere, which is at least probable regarding a certain number of shooting stars; and new facts which bear on the point, seem expressly to give this indication.* The different celestial bodies are not mere copies of each other, but are to be considered as distinct and individual; and we cannot arbitrarily transport, from the one to the other, by simple analogies, and without positive experimental proof, any relation which is not a necessary consequence of the law of attraction, or of their first common origin, and especially with regard to bodies of different orders, such as the earth and the moon. It seems much more natural to assign the origin of annular mountains to the action of simple elastic forces, without any very high temperature; these forces being capable, since the formation of the lunar globe, of acting with great activity, and without its being repeated at a later period in any thing like the same proportion.

When the existence of inhabitants is admitted, in natural philosophy, not only in the moon, but also in all other celestial bodies, this supposition is grounded essentially upon the conviction which a reflecting being must make, that there will be the greatest possible conformity throughout creation for the most exalted purpose. It is this conviction that urges us to admit the existence of sentient beings whenever we conceive it is possible, since that which is living fulfils a more exalted destiny than that which is not.

If observations demonstrate that the general conditions of *habitability* are satisfied, and that many of them are of the same nature as with ourselves (as the relations of rotation and of density which are common to the earth and the inferior planets might induce us to believe), the probability that these bodies are inhabited would be decidedly increased. But if, on the other hand, there is found in other bodies a total absence of points essential to the existence of the inhabitants of the earth, or an immense difference in this respect, whether in quantity or quality, we should then be forced to exclude the possibility of the existence of living beings analogous to ourselves. The exposition which has been made of the physical circumstances, both general and particular, which exist in the moon, sufficiently prove that she must be regarded as in these latter circumstances. Nothing is more clear than this, that we must admit at least as wide a difference among the inhabitants of celestial bodies as between the celestial bodies themselves, and no two of them, as far as our positive information enables us to judge, are precisely similar to each other. At the same time we admit, that it would be possible to draw some few isolated consequences from our observations respecting any inhabitants there might be in the moon. Thus, for example, their organs of vision would require to support a much stronger light, and infinitely greater contrasts than ours. But we affirm, in a word, that we shall never succeed in forming a complete estimate of the corporeal constitution of beings living upon this celestial body from merely isolated considerations; and, in our opinion, such researches can never be principal objects of any future observations.

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* We here particularly allude to the periodical return of a great number of remarkable shooting stars, which takes place on the nights from the 12th to the 14th of November; this fact indicating that the region of space which the earth traverses at this epoch, forms a part of a great zone (perhaps annular), in which these masses are accumulated; at all events, the position of the moon relative to the earth, has no concern in the appearance of these meteors. [Note by the Authors.]

Although in this and the preceding number of the Journal we have been able to supply only a very incomplete idea of the selenographic labours of Messrs Baer and Mädler, we believe we have said enough to demonstrate their extent and importance, and to enable any one to appreciate the service they have rendered to astronomy by the publication of their chart, and by their description of the moon. Previous to their investigations, we were ignorant of the precise position of nearly all the notable points of our satellite. A general chart of its visible portion had never been traced with the required accuracy; and no one had ever supplied a topography, at once faithful and detailed, framed only from careful observations, and free from the illusions and preconceptions of systems. We may say that their work is a fundamental one for the object to which it relates, and that it has considerably increased the extent of our positive information, by giving it a more precise, and, at the same time, a more methodical and useful direction than had previously been thought of, and it must serve as the starting point for all future researches upon the same subject. Our authors are far, indeed, from thinking that nothing else remains to be done. In their preface, in speaking of their determination of the different parts of the moon, they remark, " However rich this part of our work may appear, it will occur to any one acquainted with the subject, that it should be considered only as a commencement and basis for future investigation. We have done what was indispensably necessary for a chart constructed upon the scale which we have adopted. We hope that measurements will yet be obtained which will be more exact, more extensive, and more numerous than our own, and that more precise methods of calculation will be employed, when several as yet unsettled questions have been determined (such as that of a real physical libration), and the complete accuracy of various elements shall have been ascertained. What relates to the measurement of the heights, is necessarily the part which is most defective as it regards the exactness of the results; and it is here the greatest quantity of work remains to be done. We may, on this point as well as others, at least offer this assurance, that we have never presented results whose precision and harmony were the result of the suppression of less concordant observations. We have principally endeavoured to take for our model, so far as it was possible, the comparative method followed by Ritter in his last work descriptive of our globe. We hope we have succeeded in preparing a comparative Geo-Selenology, for future enquirers, or a parallel of these two neighbouring worlds, which, in our opinion, ought to be the chief object of pursuit in our ulterior labours. It is thus only that these two sciences, which are even still in their infancy, although in different degrees, can be elucidated and advanced either individually, or in their respective bearings."

Geological Instructions, prepared by M. Elie de Beaumont for the French Scientific Expedition to the North of Europe.[*]

If it were the sole object of a scientific expedition, such as the one we are now about to send to the north of Europe, to obtain more information about the countries to be explored, considered in reference to themselves alone, the instructions required from the Academy might have been, in so far as regards geology, extremely short. We might have said to the naturalists of the expedition, "Scandinavia has given birth to a great number of justly celebrated mineralogists and geologists, who long ago began to describe it; illustrious travellers have explored it in all directions, and made public the results of their observations;—read these works; follow the steps of the masters of science, and strive to complete their work."

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[*] The object of this expedition, in which the King of the French takes so lively an interest, is to collect new observations in Denmark, Sweden, and Norway, the North Cape, and Spitzbergen, destined to complete those already made by the French naturalists and philosophers, during the late expedition to Iceland.

But, by using such language, the Academy, we think, would not properly fulfil its duty, and would even do injustice to the celebrated men, whose labours have rendered certain parts of the Scandinavian peninsula classical localities for geology.

The first step in geology undoubtedly consists in giving an exact description of the form and composition of the surface of a country; but the second consists in comparing together countries more or less distant. This comparison may be partly made in books, but it can only be completed by seeing the objects themselves; and it requires, at all events, the making collections of rocks, so that one may be enabled to compare them with each other. It might be very beneficial to science if a Swedish geologist, who was perfectly familiar with the great diluvial deposit of Sweden, should come and institute a comparison between it and the particular forms assumed by the diluvial phenomena in the valley of the Seine around Paris. It is comparisons of this kind that we should attempt or institute. The better a country is known by its inhabitants, the better it is always described by them, and the more likely it is to offer useful means of comparison.

Few countries have been better prepared for this purpose than the southern parts of Sweden and Norway; and it is, therefore, intended to travel through them for the sake of making comparisons.

As for Lapland, and especially Spitzbergen, they still afford ample materials for a voyage of discovery.

Having had the honour to be entrusted with drawing up the geological part of the instructions designed for the expedition which is about to set out for the north of Europe, I have thought it right not to restrict myself to the information I might obtain in the works published on these countries. Notwithstanding the obliging assistance afforded to me in my researches by the naturalists of the expedition, and the kindness of M. Eugène Robert in particular, who communicated to me the notes which he himself had made from a great variety of sources, important points might still have escaped us. I therefore addressed myself to M. Leopold de Buch who, about thirty years ago, himself brought the light of science to bear on these countries which it is proposed to submit to new investigations; and in the following instructions I shall always place in the first rank the indications of this illustrious traveller.

1. *Hypersthene Rocks.*—Among the most important rocks to collect, M. de Buch mentions the *hypersthene* rocks, which give a particular character to the maritime portion of the great chain of the Kiölen. The hypersthene sienite is a very large granular rock, and occurs in inconsiderable chains, which rise to the height of several thousand feet. It is particularly in the environs of Bergen that it presents itself in colossal forms, and there also it is of very easy access. The Samnanger-fiord, six or eight leagues from Bergen, in an easterly direction, is separated from that town by a very steep chain of hypersthene rocks, which extends so far as Ous, due south of Bergen. A similar chain has been discovered by M. Esmark, near Töns on the Glommen, in the vicinity of Röraas, that is to say, quite in the interior of the country.

These hypersthene rocks are again met with on the Alt-Eid, in the 70th degree of latitude, and, finally, they occur anew at the North Cape, but not on the promontory itself: it is necessary to penetrate into the interior of the isle of Mageröe, where I have seen them, says M. de Buch, especially on the heights of Honigvoogeid. The whole profile of the beds from Kielvig to the North Cape is very curious, and is well worthy of being examined with particular attention.

For some time past an English company has been working some copper-mines in Refsboten near Alten, about the 70th degree of latitude; and I believe also, says M. de Buch, in a hypersthene rock, which is very fine granular.

The study of these very fine granular hypersthene rocks would be very interesting. M. Gustave Rose, in describing the veins of very fine granular hypersthene sienite which traverse the metalliferous deposit of Schlangenberg in Siberia, has already remarked, how difficult it is to recognise them. When their characters become indistinct they are generally confounded with trap-rocks. But it is at present alleged, and this very circumstance is a new example of it, that the trap series contains rocks of a very varied composition, although it is always equally indiscernible.

It would be desirable, that those very fine granular hypersthene-rocks should be cleared of the confusion in which they are at present involved, and this might perhaps be accomplished, by studying carefully the series of steps through which the hypersthene-sienite passes, as it becomes more fine granular. Good suites of specimens illustrating these transitions would be very useful.

2. *Trap-Rocks.*—What I have recommended in regard to the hypersthene rocks, may be recommended, I believe, in a general way, in regard to all the trap-rocks of Sweden. When Cronstedt and Wallerius first began to call attention to these rocks, whose name they derived from the Swedish word *treppa*, signifying a stair, they could only characterise them by simple external appearances. It is certain, as I have just stated, that these same appearances are exhibited by many rocks of a different composition. In general, this composition is indiscernible; but, by carefully examining the trap-hills, some parts might, perhaps, be found in which the concretions are larger, and from which we might procure specimens capable of being submitted to the test of microscopic analysis, so advantageously employed in regard to volcanic rocks, by M. Fleurian de Bellevue, and especially by M. Cordier. Since modern researches have taught us to consider hornblende, augite, and even felspar, as only groups of species, the mineralogical analysis of trap-rocks has become absolutely necessary for the science.

True basalts, perhaps, occur among the trap-rocks of Sweden. Different authors have pointed out their localities, as, at Kinnekulle on the banks of the Wenern lake, and in the hills in the vicinity of Svebesholm and Hör in Scania.* Are these true basalts, resembling in all points those of Auvergne? We should wish to assure ourselves of the fact by comparing together good series of the rocks from the two countries.

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* Carte géognostique des parties moyennes et méridionales de la Suède; par M. Hisinger.

3. *Euphotide and Serpentine Rocks.*—Euphotide rocks have also been more than once mentioned as occurring in Norway and Lapland. A part of these rocks were assuredly nothing else than hypersthene-sienites, in which the hypersthene had been mistaken for a diallage; but do true euphotides not occur in Norway? It would be of importance to prove it. Serpentines have been mentioned as occurring in Norway and Sweden.* It is known, that there is a singular affinity between euphotides and serpentines; does an affinity of the same kind exist between the hypersthene-sienites and certain serpentines, and is the substance called serpentine the same in both cases? It would be of importance to prove it.

4. *Granite of Rapakivi.*—Among the rocks to be examined in the north, with reference to their composition, there is also a species of granite called Rapakivi, which occurs in various parts of Finland, and particularly, according to Acerby, two miles to the north of Uleaborg. If there is an opportunity, it would be right to make some carefully formed collections of it, by means of which all doubts relative to its composition might be removed.

5. *Rock Formations of Christiania, &c.*—The environs of Christiania present, as it were, a vast museum of rocks as beautiful as they are various, whose mode of occurrence exhibits a multitude of curious circumstances, and which are as well fitted as the basalts and trachytes of Auvergne, to be taken as types in the description of other countries. It will be extremely useful to possess good collections of them. I shall mention particularly the melaphyres, the porphyries, and especially the zircon-sienite, of which M. de Buch remarked as being superimposed on a sedimentary formation. A section towards Sennesio shews, says M. de Buch, the whole succession of these rocks. The quarries of Aggers-Kirke are in large veins of melaphyre, and in particular, of hypersthene-rocks. The innumerable and very remarkable modifications of the melaphyres, are well seen in crossing Krogskov from Rärum towards the Holsfiord. Epidote

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* Reise nach dem hohen Norden, von Vargas Bedemar. Frankfurt, 1819. Hisinger, Mineralogische Geographie von Schweden. Freyberg, 1819. *Vid.* also Gœa Norwegica, Christiania, 1838.—Edit.

occurs there in veins, and, in particular, pretty often in the interior of the crystals of labradorite, which the crystals of epidote have evidently acted on, to allow of their own formation and enlargement. Some geodes in this melaphyre contain bitumen. Krogskov ought to furnish an ample harvest.

The escarpements of Holmestrand shew a passage from melaphyre to basalt. Holmestrand ought to detain the person intrusted with making the collections for several days.

The zircon-sienite occurs in its most characteristic forms in the vicinity of Laurvig, and also in the direction of Stavern. This beautiful rock here extends over entire square leagues; and it is therefore unnecessary to point out more particularly proper places for collecting specimens of it.

It again appears at Egersund, to the south of Bergen, where its extent has not been examined, although this would be well worth the trouble. Farther on towards the north it is not found.

I shall not confine myself to merely recommending the expedition to search for all the varieties of rocks which I have mentioned, and to make complete collections of them. The remarkable circumstances of the mode of occurrence of these rocks, the manner in which they often intersect the adjacent rocks, or disturb or cover them; the modifications of texture, and sometimes even of composition, which the latter exhibit near the point of contact, ought to be studied in detail and illustrated by good sets of specimens; they ought also to be figured.

Our two members, MM. Alexandre and Adolphe Brongniart, during their travels in Norway and Sweden in 1824, observed, especially in the environs of Christiania, numerous masses of eruptive rocks, such as, for instance, melaphyres injected among pre-existing rocks. M. Adolphe Brongniart has made drawings of the positions and the mutual penetrations of these rocks, which are both picturesque and geological, and which I would recommend to the imitation of the naturalists and artists of the expedition.

It is known how important, in a geological point of view, those veins have become which were discovered by Hutton in Glen Tilt, in Scotland, piercing through the superimposed limestone. Norway abounds in phenomena of penetration no less curious. MM. Naumann and Keilhau have pointed out a great number of them, and figured several,[*] but always in drawings on a small scale, which undoubtedly give an idea of the light which science may derive from these localities, but which are not always sufficient to allow of their being compared in a satisfactory manner with the appearances of the same kind which exist in other countries; as, for instance, in the Alps and the Pyrenees.

Drawings on a sufficiently large scale, in which these classical localities should be faithfully represented, including partly at least, their picturesque beauties, and in which the entanglements of the rocks should be represented, as M. Adolphe Brongniart has done it, by a suitable use of colours, would form most interesting illustrations for the geologist.

It is now proved that the melaphyres, the porphyries, the sienites, and even the granites, as well as a number of other analogous rocks, have crystallized by cooling. These rocks, consequently, are products of the mysterious laboratories which our globe has possessed at all times. In this point of view they resemble the volcanic products of the present epoch. But this apparent similarity of origin does not go so far as to establish their identity, for there are numerous and important differences. The time is arrived for science to fix these differences, and it will be chiefly by an attentive, detailed, and even minute examination of the phenomena of penetration, such as we have just been treating of, that we shall be able to discover the interpretation and the extent of these differences.

The beds of gneiss present throughout their whole extent very remarkable contortions and wavings, which are extremely striking as they are seen exposed to view for a great distance, and quite bare, without being concealed by vegetation or forests. "A good artist," writes M. de Buch to me, "would fill a very large portfolio with these phenomena," and it is to be desired that this should be accomplished.[†]

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[*] Naumann. Beyträge zur Kentniss Norwegen's. Leipzig, 1824. Keilhau. Darstellung der Uebergang's formation in Norwegen. Leipzig, 1826. Gœa Norwegica, Christiania, 1838.

[†] The granular white statuary marble is generally contorted like the gneiss. It is seen at Salthellen, near Sulboefiord, to the south of Bergen, in the six-

The phenomena of the stratification, the wavings of the beds, the dislocations, the relation of their position to that of the masses of eruptive rocks, may be studied more on a large scale, and this study may raise new questions.

6. *Two great series of Dislocations in Scandinavia.*—If we cast our eyes over sufficiently detailed maps of Norway and Sweden, we see pretty evidently that the principal features of the eastern slope tend to two different directions whose combination determines all the forms of the country.

The first of these two directions, which is particularly perceptible in the disposition of the isles of Loffoden, in that of the arm of the sea, and of the lakes in the vicinity of Drontheim, and in that of the range called the *Dovre Field*, between Drontheim and Christiania, runs N. E., and E. NE., cutting the meridian of Christiania at an angle of a little above 60°. It is at the same time cut at a very marked angle by the largest of the small chains of the Scandinavian Alps. The most considerable of these chains, known by the name of Kiölen, commencing at the north-eastern extremity of the Dovre Field, separates Sweden from northern Norway; and after dividing at its north north-eastern extremity, among the different gulfs of Finmack, it terminates in the Frozen Ocean at Sverholt, between Laxefiord and Porsangerfiord, and at Nord-Kyn, between this last bay and Tannafiord.

The existence of these two principal directions has led me to conjecture that there must have been two principal series of dislocations in Scandinavia; the first of which would seem to belong to the great system of dislocations which has affected

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tieth degree of latitude; on the isle of Wyck, near the great isle of Sartaroe; and at Hope Holm, one league's distance from Bergen, near Tiosanger.

A search for *beds of dolomite in the mica-slate* is also to be recommended. "There will be seen and found in them," says M. de Buch, "many minerals still unknown in these localities: green tourmalines, rubies, kyanites and apatites. These dolomites are found at Casness, in the sixty-ninth degree of latitude, where they are of great extent. They afterwards make their appearance on the isle of Seugen, in the environs of Kloiven, where the tremolite is very beautiful; at Lenwig, to the south of Tromsöe, in the 69½th degree of latitude; at Benoejard, still nearer Tromsöe, with abundance of Staurotide; and lastly, on the isle of Tromsöe itself. These beds form a great part of those mountains, and would very probably be found high up the Fiords.

the oldest stratified deposits throughout the whole of Europe; whilst the second, judging from the direction of the Kiölen chain, appears to me to belong to the epoch of the *soulèvement* of the western Alps. These conjectures may give rise to the inquiry as to whether there was, in the north, a first *soulèvement* of very old granite, which might have given rise to the first system; a final *soulèvement* of hypersthene-rocks, which might have given rise to the Kiölen; and whether, during the very long interval between these, there appeared the zircon-sienites, the porphyries and the metaphyres, which seem only to be connected with orographical changes of a less important description.

7. *Progressive Rise of the Land above the Level of the Sea.*—In proposing these different questions I cannot expect that they should all be speedily answered, on account of the small number of sedimentary formations which are visible in Scandinavia; but if we cannot supply all the deficiencies of science in relation to those ancient phenomena, we shall perhaps be compensated by the observations which the expedition will have it in their power to make on the phenomena which, in the present times and under our own eyes, prove the mobility of the crust of the globe. Those phenomena, whose traces are observed on the coast, are particularly to be recommended to the attention of an expedition which will have a Government vessel at its disposal. I allude to the change of the level observable on many parts of the coast of Sweden.

Every one knows that certain points on the coasts of that country are progressively rising above the level of the sea which washes them. I shall not mention the ancient observations of Celsius and Linnæus, the marks cut on the rocks of the Baltic and the Cattegat, the conclusions deduced by Playfair, the observations of M. de Buch, the incredulity with which they were at first received, and the reiterated observations which set all doubt at rest. M. Arago, in the *Annuaire du Bureau des Longitudes*, and Mr Lyell in the Philosophical Transactions, have given to this class of facts all the celebrity which they so justly merit.

But what is not so generally known, and what renders the phenomena still more curious, is, that not only does it not take place over the whole coast at the same yearly rate, but that certain points, in place of a progressive elevation, experience a gradual lowering of level, whilst others remain stationary.

The points which exhibit these three different states are all equally worthy the attention of observers, as the three classes of observations would verify each other. When we see that, on the same coast, certain points are sinking, whilst some remain stationary, and others are gradually rising, we cannot dread any error in the observations; and these observations, whilst they prove that the crust of the earth is mobile—prove also that it is sufficiently flexible to allow of points near each other being moved in contrary ways.

In order to explain the matter more clearly I shall mention some local observations.

Baron Hermelin, to whom we are indebted for a mineralogical description and a map of Lapland, wrote in 1804,* that between Seivits and Mikkala, and between the latter and the town of Torneo, are two bays, whose depth diminishes from year to year, and which, since some stone bridges were built across them a few years ago, are almost completely dry. The French academicians reached Torneo by sea, in 1736, and, in more ancient times, large vessels could go so far as the town; but in the present they are obliged to remain at the southern extremity of Björkör, on account of the small depth of water.

8. *Gradual sinking of the land in Scania.*—It thus appears that the phenomenon of progressive elevation, so well known on the coasts of Sweden, from Calmar to Gefle, extends as far as Torneo. But it does not take place in Scania: the coast of Scania, on the contrary, is gradually sinking.

In 1749, Linnæus had measured the distance between the sea and a rock near Trelleborg; Professor Nilsson has found, that this distance is at present 100 feet less than it was in the time of Linnæus. In a great number of the ports of Scania, there are streets which are below the level of the high-water mark of the Baltic; some are even under the level of the lowest tides. At Malmæ, the sea sometimes covers one of the streets of the town, and they have discovered, by means of excavations, the surface of an ancient street, eight feet lower. At Trelleborg and at Skanœr, there are streets several inches below highwater mark; whilst at Ystad there is a street exactly on a level with the sea. It is clear that these could not have been built in their present position in reference to the sea.

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* Hermelin's Minerographie von Lappland und Westbothnien, p. 138.

9. *Stationary condition of parts of the coast of Norway.*—In another part the coasts of Norway appear to be stationary; at least M. Eugène Robert, who travelled through Scandinavia, mentions that the sides of the Christianiafiord, would appear to have remained stationary for the last two hundred years, to judge from a pavement of the ancient town of Fredericksvaern (burned down since that period), which is still on a level with the sea at the harbour.

Mr Everest, in his Travels in Norway, informs us, that the small isle of Munkholm, which is an isolated rock in the harbour of Drontheim, presents a conclusive proof that the land in that quarter has remained stationary during the last three centuries. The superficial extent of this isle does not exceed that of a small village, and an official survey has proved that its highest point is 23 feet above the mean high-water mark. A monastery was founded on it by Canute the Great, in the year 1028; and thirty-three years before that time, it was used as a place of execution. According to the mean rate of elevation in Sweden (about forty English inches each century), we should be forced to suppose that this isle was 3 feet 8 inches below the mean high-water mark, when it was fixed upon as a site for a monastery.

It would be extremely interesting to trace, at some future period, on the map of Scandinavia, the respective limits of the ascending, the descending, and the stationary zones. Nothing should be neglected which can lead to a result so important for terrestrial physics and for geology.

10. *Beds of recent shells above the present level of the sea.*—Certain geological facts also shew, that the relative level of the land and sea has varied in several parts of the north of Europe at a recent geological epoch. I allude to deposits of shells, often argillaceous, which are observed in certain parts of Sweden and Norway, at different heights above the sea;

deposits which may be compared, in their nature, with the *Fahluns of Touraine*, and the *Suffolk Crag*, but which are probably more modern.

Every one is acquainted with the curious observations made in 1807 by M. de Buch, and verified since that time by M. Brongniart and Mr Lyell, on the deposit of marine shells of species now living, situated at Uddevalla in Sweden, near the frontiers of Norway, 70 metres (about 230 feet) above the sea.*

Deposits of the same kind have been observed in the environs of Stockholm, as also at Orust, and on the banks of the lake Rogvarpen.

The environs of Christiania also exhibit examples of this kind. Professor Keilhau has observed them there, even 600 feet above the sea. M. Eugène Robert, in his journey through Norway and Sweden last year, has likewise stated different facts of the same kind. He remarked, for instance, between Drammen and Christiania, on the side of the road at Raunsborg, a black fetid limestone, with Terebratulæ, full of shells of the *Saxicava rugosa*, which have perforated it at an epoch when the sea reached this point, now elevated 500 feet above its level.

Facts of this kind are valuable, because they are clear and undeniable. We may expect to find examples of them throughout the whole extent of the coasts of Norway.

Marine shells of species now living, have been collected in places far up the country, near Drontheim. According to M. de Buch and M. Ström, deposits of this nature exist at an elevation of more than 400 feet above the sea in the northern part of Norway. According to M. de Buch, beds of shells are observable on the isle of Luröe, even within the Polar Circle, and at Tromsöe, about the sixty-ninth degree of latitude.

They also make their appearance again at Spitzbergen. According to Professor Keilhau, there is found in the Stans-Foreland (one of the large islands which, taken together, form Spitzbergen), at the distance of nine and a half miles from the sea, and 100 feet above its level, a bed of alluvial clay containing bivalves, and analogous to those on the coasts of Norway.

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* Linnæus, in his Travels through West Gothland, gives an interesting account of the Uddevalla formation, and a plate, with figures of several of the more important fossil shells.—*Edit.*

On the *Thousand Islands* which border the south coast of Spitzbergen, we find accumulations of the bones of whales.

According to Pennant, the shore of a low island situated to the east of Spitzbergen, almost opposite to the entrance of the Waygat, is formed of an ancient concretion of sand, of bones of whales, and of trunks of trees or of floated wood.

However, these deposits of shells, which are so extensively spread over the coasts of the north of Europe, do not invariably occur there.

According to the data furnished by Professor Nilsson to Mr Lyell, and stated by the latter in his address to the Geological Society of London in February 1837, no beaches of shells analogous to those of which we have been speaking are found in Scania. It is also known that this phenomenon is almost entirely awanting in the middle part of Europe, or is only observable at a very small height.

The geological facts which I have just mentioned have often been connected with the present phenomenon of the gradual elevation of certain parts of Sweden; but there is nothing to prove that the raising of beds of shells to a high elevation is the result of a slow and gradual process. Their general appearance would seem, perhaps, more in harmony with the idea of a sudden rising. This point, however, will be an interesting subject of research for the expedition.

But, what already appears certain is, that the spheres of activity of the two phenomena (the present and the ancient change of level) are very different from each other. M. de Buch, who has always regarded the two phenomena as very different, has shewn, in a decisive manner, that the phenomenon of the elevation of Sweden is unconnected with those parts of Norway which are covered by the beds of shells of which we are speaking. We see, he writes to me, at Luröe, runic stones, placed on beds at so slight an elevation above the sea, that there would have been no foundation on which to place these stones, which are traceable to a remote antiquity, if the rate for Sweden, of four feet of elevation in each century, were applicable to Norway.

I need scarcely add, that all the facts of this kind, and all the remarks relative to this question which the expedition could collect, would be valuable acquisitions for science.

Besides, it would be of consequence to possess good collections of those recent fossil shells which are spread over so many parts of the surface of Sweden, Norway, Lapland, and Spitzbergen, and also collections of the shells which are now found living in the neighbouring seas, in order to be able to appreciate exactly the degree of resemblance which they have to each other, and the greater or less changes which may have taken place in the seas since they became fossil.

A certain creek in Spitzbergen called *Shell-Bay*, might perhaps furnish, in the way of fossil or living shells, some useful objects for this comparison.

It would also be interesting to find in these deposits bones of quadrupeds or of cetaceous animals, which have not yet been noticed, except at Spitzbergen; a deficiency the more remarkable, as the seas of the northern inhabited countries swarm with so great a number of whales, seals, white bears, and other large animals, and as Sweden, Norway, and Lapland, contain a great number of rein-deer, wolves, bears, wolverines, and other quadrupeds, whose bones are certainly buried, in the present times, in the shore deposits.

11. *Beds of Fossil Infusoria.*—Amongst the modern deposits which should be recommended to the attention of the naturalists of the expedition, we must not forget that fossil farina, chiefly composed of siliceous shields of infusoria, often analogous to living species, which the Laplanders sometimes mix with their food. It has been found near Umea, at Degerford, and in Finland. It would be interesting to ascertain the mode of occurrence of this siliceous deposit of organic origin, and to possess collections of all its varieties, and of whatever is associated with it.

12. *Transition and secondary formations.*—Most of the sedimentary formations which have been observed by geologists in the centre and south of Europe are awanting in Scandinavia. There is reason to think that, during a great part of the tertiary and secondary periods, this country formed part of a large island or continent; and the small patches of secondary formations which we find in it, are therefore of the greater interest in a geological point of view. I shall mention particularly, in reference to this, the small patch

of jurassic formation which M. Hisinger, in his excellent geological map of southern Sweden, represents in the southern part of the island of Gottland. This is one of the most northerly points in which this formation has as yet been shewn to occur. M. Hisinger, in his work entitled *Læthea suecica*, mentions, as occurring in the island of Gottland, fossils which prove indisputably that the lias occurs there, as, for instance, the *Gryphœa arcuata* the *Lima gigantea*, and the vertebræ of *Ichthiosauri*. It would be interesting to possess a collection of the fossils of this deposit, in order to compare them with those entombed at the same period in more southern climates.

I should also recommend to the notice of the naturalists of the expedition, the deposit of coal which occurs in the island of Bornholm, and which one is inclined to refer, like that of Brora in Scotland, to the jurassic formation. A series of the animal and vegetable fossils by which this combustible is probably accompanied, would be interesting.

But, if the tertiary and secondary periods are only represented in the Scandinavian peninsula by patches of inconsiderable extent, we are partly indemnified for this by the great development of the more ancient deposits which are called *transition*. The deposits of this period cover whole provinces, and occur most frequently in almost horizontal beds, which are as easily examined as the jurassic formations of France and England. These formations are filled with numerous and well preserved fossils. It would be interesting to possess originals of the remarkable figures published by M. Hisinger in his *Læthea suecica*; and it would, perhaps, still be more interesting to possess the fossils of the transition-formations of Norway. As yet no special description of these last has been published, not even of those which are contained in the slate and limestone of Christiania.* At Christiania itself, M. de Buch informs me, in going up the *Aggers-Elv*, towards *Aggers-Kirke*, we find many orthoceratites in this limestone. The alum mine of Opslo includes ellipsoidal globules of limestone, each of which contains a petrifaction; but the greatest number of these organic bodies is found in the parish of Eger, between Christiania and Konigsberg. The best known places in this respect are the farms of Raae, of Soulhong and Saasen, on the western banks of the lake of Fiskrem, where the orthoceratites and trilobites are heaped together in thousands. It would be desirable to fill some boxes with these productions, in order that they might be carefully examined in Paris.

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* In Keilhau's Gaea Norwegica there is a valuable memoir on the Trilobites of Norway.—*Edit.*

I have never heard, adds M. de Buch, that organic bodies have been found in the slates of Hedemarken or of the Hadelaneb; it would therefore be so much the more interesting to discover even slight traces of them in these districts.

These collections will serve to establish the relations which doubtless exist between the different *étages* of the transition-formations of the Scandinavian peninsula, and those of Wales, which Messrs Murchison and Sedgwick have divided into two great systems, the Silurian and the Cambrian.

But if the motives which I have mentioned render it desirable to procure good collections of fossils from the southern parts of the Scandinavian peninsula, a greater interest is attached to those which might be collected in the high latitudes which the expedition is to explore. Sedimentary formations, which may be supposed analogous to those of the south of Sweden, cover a portion of the country between Torneo and the North Cape; the valley of the river Torneo, near that town, and the valley of the Alten near Aaltengaard, traverse these formations; but what particularly may make us expect discoveries of the greatest interest, is the circumstance that parts of Spitzbergen and the neighbouring islands are formed of stratified rocks of a somewhat analogous appearance.

13. *Geology of Spitzbergen and Bear Island.*—I shall state in a few words the details which have been given to us on the geological constitution of Spitzbergen, by several well-informed travellers, such as Pennant, Lord Mulgrave, and especially Captain Scoresby, and Professor Keilhau of Christiania.

The western part of Spitzbergen has a nucleus, a chain of bold mountains which rise to a considerable height. Their jagged forms, to which the name of Spitzbergen is undoubtedly due, exhibit a ridge of crystalline rocks, analogous to those of the Swiss and Scandinavian Alps. Their massive aspect sometimes resembles granite ; in other parts they are stratified, but in highly inclined beds. They begin at the southern point in 76½° of latitude. Here they consist of mica-slates and quartz-rock in vertical beds, running NE., SW. These rocks form the hilly country surrounding *Horn Sound* and *Bell Sound*, and they continue still more to the north. Horn Sound owes its name to a peaked mountain composed of the same rocks. This mountain, the highest of those measured by Captain Scoresby in 1815, has, according to him, an elevation of 4395 English feet.*

Stratified deposits of a more recent date also occur in these parts, and they would appear, as in southern Sweden, to rest unconformably on the old crystalline rocks. Captain Scoresby mentions a curious example of this. We see, says he, at the extremity of King's Bay, a very regular and magnificent work of nature. It consists of three rocky pillars of a regular form known by the name of the *Three Crowns*. They rest on the ridge of other hills. Each one has, as its base, a table or horizontal bed of rock ; on this we find another of the same form and height, but of a smaller area. This continues to a third and fourth bed, and so on, each of the succeeding strata being smaller than the one on which it rests, until the whole forms a pyramid of steps, as regular as if it had been constructed by art.

Captain Scoresby mentions the occurrence of fine marble in King's Bay, and the name of *Limestone Bay*, given to one of the indentations of the coast, leads one to presume that calcareous beds are found there. Limestones, sandstones, and even gypsum have been observed at the base of the mountains, in the bays or fiords which intersect the coast, and in the islands which border it. Carboniferous deposits of indifferent quality have been found there, extending to the 79th degree of latitude, and of very easy access. In 1826, 60 tons of coal were exported from Ice Sound, situated in the 78th degree. The name of *Cannel-coal* has been given to this combustible ; and this would seem to imply that its aspect resembles that of one of the combustibles of the coal-formation.*

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* In our account of the primitive rocks collected in Spitzbergen by Captain Parry, the following are enumerated, viz. granite, gneiss, mica-slate, hornblende-rock, limestone, dolomite marble, quartz-rock, chlorite-slate, and clay-slate. The transition rocks met with were chiefly clay-slate, quartz-rock, and limestone.—*Edit.*

The different localities which I have just mentioned are situated on the western coast, which is the most frequently visited, but which, according to all appearance, is not the richest in sedimentary deposits; for it is partly formed of the chain of crystalline rocks, of which I previously spoke, whilst, as we proceed towards the east, the country becomes lower and more flat.

The SE. part of Spitzbergen is formed by a large detached island called Stans-Foreland. According to Professor Keilhau, the western coast of this island presents, between the 77th and 78th degrees of latitude, a granular trap-rock, covered by alternate beds of fine sandstone, arenaceous slaty marl, compact siliceous limestone, and trap. This formation appears to extend to the 80th degree of latitude, and it probably prevails over the greatest part of Eastern Spitzbergen.

According to the observations of the same geologist, *Cherry* or *Bear Island*, situated between the coast of Lapland and Spitzbergen, is entirely composed of secondary sandstone, and horizontal shelly limestone *(Calcaire coquillier.)* This sandstone contains a bed of coal from 2 feet to 4 feet in thickness.

According to Captain Scoresby, lead-ore has been found in the same island in veins at the surface, as well as specimens of native silver, and *coal of a good quality*.

It would be very interesting to have a good collection of the formations of which this island is composed, and which is perhaps only an intermediate link connecting the sedimentary formation of the Stans-Foreland with that in the environs of Alten in Lapland.

How curious it would be to find madrepores in the limestones of Cherry Island and of Spitzbergen;† and to discover stems of *Equisitaceæ* and arborescent ferns associated with the beds of coal of these frozen regions. The discoveries made at

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* In the Appendix to Captain Parry's "Attempt to reach the North Pole," it is stated that the only coal he met with in Spitzbergen, was *brown coal*.—*Edit.*

† In the collection of rocks presented to us by Captain Parry from Spitzbergen, we found *madrepores* in the limestones, as stated at page 227 of "The Attempt to reach the North Pole."—*Edit.*

Melville and Ingloolick islands during Captain Parry's expedition, render this discovery probable, without diminishing the interest which would be attached to it. To prove by numerous facts that madrepore reefs were formerly able to exist within from 10° to 15° of the pole, that arborescent ferns could live and propagate themselves in a region from which the sun is absent during several months of the year, would be the completion, and in some measure, as it were, the keystone of one of the most interesting classes of geological facts—of one of those which prove in the most satisfactory manner that the surface of our globe has undergone immense changes.*

Certain deposits of wood, which, according to some observations, are found on the shores of Spitzbergen, would also possess an interest, although not of so high an order; perhaps they might furnish the proof that the Gulf Stream, which so often casts the productions of Mexico on the shores of the British isles, of Norway, Iceland, and even of Siberia, casts them also on those of Spitzbergen. As a motive to researches in regard to this subject, I may mention the name of *Wood-bay*, which forms one of the indentations of the northern coast of Spitzbergen, between the 79th and 80th degrees of north latitude, in a country in which a few annual plants a foot high find difficulty in growing.

Perhaps also the *Surturbrand* or lignite of Iceland will be found in Spitzbergen.

These two phenomena, and especially the first, have nothing in common with the corals and the tropical plants, whose geological position would announce that they grew in the country itself.

14. *Scandinavian Diluvium.*—Among the geological phenomena presented by the north of Europe, one of the greatest, the most curious, and the most important for the general ques-

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* In the collection of rocks from Jameson's Land, in Greenland, presented to us by Captain Scoresby, the discoverer of that country, we found, in a series of specimens from Neill's Cliffs, a pretty ample display of rocks of the old coal-formation, or that which rests immediately on the mountain limestone. Neill's Cliffs are situated in the southern extremity of Jameson's Land, in about 70° 30′ north latitude. *Vide* "Scoresby's Journal of a Voyage to the Northern Whale-Fishery."—*Edit.*

tions which geology inquires into, and of which it often long awaits the true solution, is the phenomenon known by the name of *Scandinavian Diluvium.* It is known that, from the shores of Northumberland to the environs of Moscow, the plains of England, the Low Countries, Denmark, the North of Germany, Poland, and Russia, are covered by an immense number of blocks, often of prodigious size, of different rocks whose *analogues* only occur on the other side of the Baltic Sea, and which must have been transported from the mountains of the north to their present position by causes, the exact determination of which is one of the most beautiful problems of geology.

All the blocks, the gravels, and the sands set in motion by this problematical cause, did not arrive at the limits which I have mentioned. Sweden is covered with them, and the traces which the phenomenon left there, have been for long the object of many observations which our fellow member M. Brongniart has partly verified, and which he summed up in a memoir read to the Philomatic Society the 12th April 1828.* Since that time the observations have continued; M. Sefstroem has been of late particularly occupied with them, and the conclusions at which he has arrived are stated in a letter from M. Berzelius to M. Dumont d'Urvile, inserted in the *Compte Rendu* of our meetings for the month of August last.†

Traces of this phenomenon also occur in Norway, and M. Eugène Robert, in the tour which he made last summer, found traces of it in the neighbourhood of Christiania. Nevertheless there have been hitherto discovered much fewer traces of this diluvial phenomenon in Norway and Lapland than in Sweden. It will be of importance to ascertain if the transported materials form also there those long stripes in the form of dikes running N.NE.—S.SW., called in Swedish *ôse* or *sundosar*, and if they were always spread over the surface of the *fahluns* or shelly clay of which I have already spoken.

One of the most curious circumstances connected with the phenomenon of which we are speaking, is the polished furrows hollowed out on the surface of the rocks, which Saussure, in speaking of the great *debacles*, whose traces he discovered in the Alps, has designated as the ruts caused by the passage of the transported blocks, and on which the observations recently made by M. Agassiz in the environs of Neufchâtel have anew contributed to fix attention.*

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* See *Annales des Sciences Naturelles,* vol. 14, p. 5.

† *Comptes Rendus,* vol. 5, p. 341 (Meeting of the 28th August 1837). *Vide* also Edinburgh New Phil Journal, vol. xxiii. p. 69.

A French philosopher, well known to the Academy, Count Charles de Lasteyrie, an old travelling companion of Dolomieu, whilst travelling himself in Sweden in the beginning of this century observed similar furrows, and, some time afterwards, one of the most illustrious geologists of the Scotch school, Sir James Hall, observed analogous ones on the Corstorphine Hills, two miles to the west of Edinburgh. Messrs Buckland and Sedgwick have also discovered them in other parts of Great Britain. M. Brongniart, during his travels in Sweden, verified, along with M. Berzelius, the reality of these appearances.

"We found," says he in the memoir already quoted,† "our celebrated and learned travelling companion, M. Berzelius, disinclined to admit the constant occurrence of these furrows, until, struck by their abundance and distinctness towards the descent of Hogdal, he could not resist the evidence of so remarkable a phenomenon.

"Since then, this phenomenon has not ceased to be studied; the Academy has been able to judge of it by the letter already quoted from M. Berzelius to M. Dumont d'Urville; and I shall now give, in addition, the following extract from a letter which M. Berzelius did me the honour to address to me, the 8th November last, through M. Eugéne Robert.

"I send along with this letter, says M. Berzelius, a stone, taken from the surface of the porphyritic mountains of Elfdalen in Dalecarlia, which bears marks of a geological revolution, whose traces my countryman M. Sefstroem has studied on our mountains, and which appears to me to deserve the attention of geologists. You are, without doubt, aware of a letter which I wrote to M. Dumont d'Urville at the request of M. Sefstroem, and which M. Arago did me the honour to read to the Academy of Sciences, and of which there is an extract in a French journal. On this account, I shall not speak at present of the ideas of M. Sefstroem.

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* Letter from M. Agassiz to the Academy of Sciences. *Comptes Rendus*, vol. 5. p. 506 (Meeting of 2d October 1837). *Vide* also Edin. New Phil. Journal, vol. xxiv. for Agassiz's Memoir upon Glaciers, Moraines, and Erratic Blocks.

† Ad. Brongniart's Notice sur les blocs de roches des terrains de transport en Suède (*Annales des Sciences Naturelles*, vol. 14, p. 17).

"As to the specimen sent herewith, it was destined to accompany the letter to M. Dumont d'Urville; but this was impossible, as the letter was sent by post. You will remark that it appears as if polished by emery in a constant rectilineal direction. All our mountains have the NE. side worn by parallel furrows, rectilineal in the direction from NE. to SW., which on the granite are often much deeper and broader than on this harder stone. The SE. side, on the contrary, still exhibits the angular outline formed at the time of their *soulèvement*.

"M. Sefstroem explains this phenomenon by a current of water and rolling stones, of which this current has left, at least with us, enormous remains. The memoir of M. Sefstroem, presented to the Academy (of Stockholm) two years ago, is to appear immediately, and will probably be reprinted in *Poggendorff's Annalen*. The engravings, rather difficult to execute, which accompany it, have been the cause of the delay in its publication."

I have the honour to exhibit to the Academy the fragment of polished porphyry of Elfdalen, mentioned in M. Berzelius's letter; and I add to it, as means of comparison, a fragment of jurassic limestone similarly polished, detached from a polished surface of great extent, which M. Agassiz shewed me last July at *Landeron* near the Lake of Neufchatel.

I should think it desirable for the naturalists of the expedition to bring back as large specimens as possible of these polished rocks of Sweden; for a much more just idea is to be formed of them from large specimens. As they have a government vessel at their disposal, they will possess facilities in this respect which will perhaps not be afforded again for long.

Many geologists have thought that masses of ice acting as rafts, or in some other manner, have performed an important part in the transport of erratic blocks. As the expedition is to visit Spitzbergen, where there are magnificent glaciers, it will perhaps have an opportunity of making useful observations on this subject.

15. *On the Glaciers of Spitzbergen.*—The mountains of Spitzbergen are covered with eternal snow throughout a great part of their extent, and vast glaciers descend in great numbers to the sea.

The valleys of this country, says Lord Mulgrave, filled up with eternal ice, are totally inaccessible, and are only distinguishable by the intervals which they cause between the tops of the mountains, or by the glaciers which mark the places where they terminate in the sea. One of the most remarkable things which can be seen in Spitzbergen, says Captain Scoresby, is the icebergs.

The most favourable situation for the formation of icebergs is, where a ridge runs parallel to the shore; and it is precisely a similar situation which, a little to the N. of Charles's Island, has favoured the accumulation of those enormous masses of ice known by the name of the *Seven Icebergs.* Each of these masses fills up a deep valley, which proceeds from the side next the sea, and is enclosed by mountains of about 2000 feet in height, and terminated in the interior of the island by the great chain whose elevation reaches from 3000 to 3500 feet, and which follows the direction of the shore. These icebergs are quite of the nature and appearance of the glaciers of Switzerland.

Each of these seven icebergs, continues Captain Scoresby, is about a mile in diameter, and perhaps 200 feet high on the side next the shore; but some of those to the south are much larger. The largest which I have seen is situated a little to the north of Horn Sound; it extends for eleven miles along the shore.

The face of the icebergs, says Lord Mulgrave, is of an emerald colour. Cataracts of melted snow rush down from different parts of the summit, and black pyramidal mountains streaked with white border the sides, and are piled rock on rock, and summit on summit, as far in the perspective as the eye can reach. Sometimes immense fragments of ice are broken off, and fall into the water with the most terrible noise. A piece of one of these masses, of a brilliant green, having fallen in this manner, and having grounded in twenty-four fathoms of water, had still an elevation of fifty feet above the water. Glaciers of this kind are common in all the Arctic Regions, and it is their breaking up which gives rise to those mountains of solid ice which abound in these seas.

As these glaciers, which are analogous to those of the Alps, must be, like them, covered by fragments of rock which have fallen from the adjacent mountains, it is to be supposed that the islands of ice, which are detached from them, must sometimes float away these blocks of rock on the surface of the ocean, and give rise to phenomena of transport, whose examination might furnish interesting terms of comparison for a part of the theory of the phenomenon of erratic blocks.

But, independently of these ordinary phenomena, we may also ask if some great volcanic eruption, taking place near the pole, may not have put in motion the polar ices loaded with masses of rock, and thus suddenly caused a great dispersion of erratic blocks? The physical possibility of phenomenon of this nature ought to give particular importance to all the rocks of eruptive origin observable in the frozen zone. Spitzbergen is not devoid of them. I have already mentioned the trap-rocks observed by Professor Keilhau in Stans-Foreland. They occur also in other places.

To the east of Spitzbergen, says Pennant, there is a very low island, almost opposite to the entrance of the Waygat. It is remarkable from being merely a part of the basaltic chain observable in many places in the northern hemisphere. It is, he adds, a kind of marble of the finest grain, of a deep black, and shining like polished steel, never occurring in beds in the earth, but standing upright in pillars with regular angles, composed of a number of parts placed one above the other, with so much exactness, that one would say they were formed by the hand of a skilful architect. Here the pillars are from eighteen inches to thirty inches in diameter, for the most part hexagonal, and forming a superb pavement or floor of marble.

According to all appearance, traps or basalts occur there; and, from some remarks of Captain Scoresby, we might be tempted to ask further if Moffen Island and Low Island, situated to the north of Spitzbergen, beyond the 80th degree of latitude, may not be formed of volcanic materials.*

16. *Volcanic Island of Jan Mayen.*—But all these foci of eruption appear to be extinguished at the present time; and it is in the island of Jan Mayen, situated in the 71st degree of north latitude, that we find the nearest volcano to the pole. May we be permitted to express the hope that the expedition will include this remarkable island within the circle of its investigations.

According to Captain Scoresby, the island of Jan Mayen is very much elongated from SW. to NE. Its length is ten leagues; its breadth three leagues. It increases in breadth at its NE. extremity, which presents the form of a lozenge, having each of its sides about three leagues in length. This space forms the base of the remarkable mountain called the Beerenberg (Bear Mountain). The SW. part of the island is joined to the NW. part by a narrow isthmus, and is itself very much elongated, whilst its breadth varies from one to five miles.

The object which particularly strikes the attention, on approaching the island, is the pointed peak of the Beerenberg, rising to an elevation of 6870 feet above the sea. It appears placed on a base, which is itself hilly, and rises to a mean height of 1500 feet.

Three places in this island, called *Wood Bay*, *Great Wood Bay*, and *Little Wood Bay*, received their names from the great quantity of pieces of decayed wood which are found there. These woods, whether floated thither or fossil, will give rise to the same questions as in Spitzbergen.

Captain Scoresby, having landed in Jameson's Bay, remarked indications of volcanic eruptions. We observed, says he, at every step fragments of lava; he also mentions trap-rocks and cellular basalt, with crystals of augite, ashes, scoriæ, vesicular lavas, and burnt clays. A hill, Esk Mount, 1500 feet high, which he ascended, presented a beautiful circular crater from 500 feet to 600 feet deep.

Another analogous crater was seen to the SW. of the first.

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* Low Island is composed of transition rocks. *Vide* "Parry's Attempt to reach the North Pole," p. 228.—*Edit.*

The appearance of the whole country announced the action of subterranean fires.

At some distance, near some large fissures which were visible here and there in the rock, they saw immense accumulations of lavas which seemed to have been ejected from these fissures.

At the end of August 1818, says Captain Scoresby, we were surprised to see near Mount Esk considerable jets of smoke proceeding from the earth at intervals of from three to four minutes. This smoke was driven upwards with great velocity, and reached a height of 4000 feet.

This eruption, and the traces of ancient eruptions remarked by Captain Scoresby, might probably, like those which sometimes happen at a short distance from the sea, near Naples or Catania, be only lateral eruptions of a principal volcano, which would be, according to all appearance, the Beerenberg itself.

This might be ascertained by an attentive examination of the structure of the whole island, and particularly of its northern part. It is probable that the snows and glaciers, which are not less remarkable in the island of Jan Mayen than in Spitzbergen, do not allow of this mountain being ascended, but perhaps the naturalists of the expedition will be able to explore those grouped around its base, and to penetrate into the crevices which they present. It will, at all events, be possible to sketch, under various points of view, this remarkable peak, whose structure perhaps bears some resemblance to that of the Peak of Teneriffe. It would also be very interesting to make collections of the volcanic productions of the island, and of the other rocks which may occur in it.

The interest which would be attached to these researches would be so much the greater, as the island of Jan Mayen is situated in the prolongation of the volcanic zone which crosses Iceland, and which has already been an object of investigation to several of the naturalists of the present expedition.

The examination of the island of Jan Mayen would complete, in a brilliant manner, the investigation of the northern Atlantic Ocean, pursued with so much perseverance by MM. Gaimard and Robert.

Remarks upon the Physical Constitution of the Arabians, who may be considered as the Prototype or Primitive Race of the Species. By M. Larrey.*

During our expedition to Egypt, at the close of the last century, I directed a large share of my attention to the study of the physical condition of its inhabitants, and especially to that of the Arabians. With this object in view, I made extensive anatomical researches into the physical constitution of many individuals, of both sexes and of all ages, whom we were in the habit of attending in particular wards of our military hospitals. I also prepared skeletons, and a great number of crania, which were deposited, along with many other objects of natural history, in my house at Cairo, into which, during my absence at Alexandria, the plague was introduced. Upon this the Commission of Health of Central Egypt ordered the whole of the contents of the mansion to be burned, and thus my collection was destroyed. But notwithstanding this misfortune, I rejoice that I had noted in my journal, and in my work *Relation Chirurgicale de l'Armée d'Orient*, the chief peculiarities of the physical constitution and of the character of these Arabs.

I shall now add to this sketch the result of the additional researches which I have made, whether individually or with the assistance of associates,† both in Egypt, and more extensively over Africa; and which specially refer to the external forms of individuals of this nation, to the structure and density of their osteology, to the conformation of their internal and external organization, and to their instinctive faculties.

This interesting people, undoubtedly one of the most ancient in the world, have their chief residence in that immense country, which, on the one hand, separates the Red Sea from the Persian Gulf, and on the other, the Mediterranean from the Indian Ocean. The mild and salubrious climate of this country presents some slight modifications, which are to be attributed to the differences of the surface, or to the nature of the soil, in each of its principal regions. Its productions are well known; and its native inhabitants and animals have a physiognomy and character which are quite peculiar, and which distinguishes them generally from all those which appear in the other regions of the globe.

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* Drawn up for the use of the Scientific Commission which is about to proceed to the French possessions in Africa.—*Compt. Rend. No.* 23, *Juin* 1838.

† Dr Guyon, Chief Surgeon of the Military Hospitals in Africa, has especially supplied me with a number of valuable observations.

The physical study of the first class among them, viz. the Arabians, has been the principal object of my researches, as I have stated in my Egyptian campaign; and this variety of our race may be easily distinguished into three different groups: 1*st*, That of the Eastern Arabs, coming from the margin of the Red Sea, or from Arabia properly so called; 2*d*, That of the Western Arabs, or the original Africans of Mauritania or of the coasts of Africa; and, 3*d*, That of the Bedouin Arabs or Scenites, the wandering hordes of the confines of the deserts.

The individuals of the first group, who abound and are perpetuated in the class of *Fellahs* or labourers, and in those of all the artisans of the whole of Egypt, and the fertile countries of Africa, are somewhat above the average stature; they are robust and well formed; their skin is sun-burned and brown, and is elastic; their countenance is oval and copper-coloured; their forehead is broad and elevated; the eyebrow is black and bushy; the eye is of the same colour, deep-seated, and quick; the nose is straight, and of medium size; the mouth is well defined, the teeth well set, beautiful, and white like ivory; the ear, beautifully formed and of the normal size, is slightly curved forwards; the auditory foramen is exactly in the same level with the external or temporal commissure of the eyelids, as generally happens in every individual of every race.* Some additional differences may be observed in the women; the graceful contour of their limbs is especially admirable, also the regular proportions of their hands and feet, and the elegance of their attitudes, steps, &c. &c.

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* These lineaments have been traced in all their accuracy by the able pencil of Girodet, in the head of an Arab Chief which I have presented to the Academy. It is also to be observed, that the external ear varies widely both as to form and size, not only in different nations, but also among individuals of the same tribe.

The second group of Arabs does not differ essentially in its physical forms from the first; and there is, moreover, a perfect analogy of character between the individuals of these two groups.

The Bedouins, or shepherd Arabs, are generally divided into tribes, which are scattered upon the confines of the fertile lands, at the commencement or upon the margin of the deserts. They live under tents, which they transport from place to place according as they find removal necessary. They have generally a very strong resemblance to the other Arabians; whilst it may also be observed, that their eyes are more sparkling, their features are usually less distinct, and their stature is somewhat inferior to that of the civilized Arabs. They are also more agile; and, though slight built, they are very strong. Their imagination is very lively, and their character is haughty and independent; they are suspicious, dissembling, and restless, but at the same time brave and intrepid. They most religiously observe the rites of hospitality. They are especially remarkable for their profound address, and for great and remarkable intelligence. They are regarded as excellent horsemen; and they boast, not without reason, of their skill in the use of the lance and javelin. Besides, they are exceedingly skilful as tradesmen and artisans.

The manners and customs of all these classes are very nearly the same. They rear sheep, camels, and horses of the rarest breed; they all speak Arabic, and profess the same religion. They all, moreover, live nearly in the same way. Their nourishment consists principally in dairy produce, eggs, and vegetables; they eat but seldom, and consume but little meat; and in general they are very sober and temperate as to drinking; and they easily support all kinds of privation. They all shave the head, and allow the beard to grow.

The women allow their hair to grow, and often colour it, as likewise the eyebrows with paint of a more or less deep brown colour, which is in no degree injurious to the hair; on the contrary, it rather strengthens it, and imparts to it a beautiful black colour. They also dye with a liquor of a golden-yellow colour, procured from the henna-plant, the circumference of the feet and hands, reaching to the points of the toes and fingers. These parts, and also the countenance of the young of the higher classes, are protected from the disfiguring effects of the small pox (when not preserved by inoculation) by means of gold leaf, which is applied to all these parts at the invasion of the malady. This practice seems to have been common to the Egyptians, and to the Arabians properly so called.*

Every individual, of either sex, wears a turban more or less rich, according to his wealth; this turban is worn in form of a circle round the head above the ears, which are slightly inclined towards the temples, so that their head appears almost spherical in form, and unusually elevated.† It is this particular form of the ears, and this remarkable elevation of the cranium, which, without doubt, have led our much respected associate, M. Dureau de la Malle, to remark that the auditory foramina were placed lower in the heads of Arabians, than in those of people of other nations; but we are convinced by the comparative examination of the temporal bones in which these foramina are placed, that their respective situation is absolutely the same in the heads of the inhabitants of all other countries.

The peculiar genius of these men led them to supply the first shepherd-kings to Egypt, as also the first astronomers, the profoundest philosophers and most able physicians; their other exploits and conquests are well known.

The perfectibility which we have thus recognised in all the internal organs, and in those also which belong to the external members of these Arabians, really announce an innate intelligence proportionate to that physical perfection, and which is, without doubt, superior, other things being equal, to that, for instance, of the people of the northern regions of the globe.

In Egypt, we have remarked that the young Arabians of both sexes imitate all the productions of our artists and artisans with the most astonishing facility; they also acquire languages with remarkable facility.

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* M. Larrey exhibited to the Academy the foot of a mummy in which the traces of this kind of gilding were apparent.

† This excentric expansion, or development of the process of ossification, which proceeds from the centre to the circumference, confirms the principles which I have maintained in the memoir I had the honour to read to the Academy upon wounds of the head, and the osteology of the cranium. The examination of the specimen here presented may convince every one of this physiological verity.

It is highly probable that the climate of Arabia, together with the sober, regular, and simple life of this race of men, who owe their birth to this rich and fertile country, have contributed to produce that perfectibility of organization, and that rare intelligence, which, in some respects, makes them altogether a peculiar race.

Independently of this elevation of the vault of the cranium, and of its almost spherical form, the surface of the jaws is of a great extent, and is in a straight or perpendicular line; the orbits likewise, wider than they are usually observed in the craniums of Europeans, are somewhat less inclined backwards; the alveolar arches are of moderate size, and supplied with very white and most regular teeth; the canines especially project but little, confirmatory of the assertion made by travellers, who have all agreed respecting the regimen of the Arabs, that they eat but little, and very seldom of animal food. We are also convinced that the bones of the head of individuals of this nation are thinner, and, as it appears to me, more dense, allowing them to be of the same dimensions as those of other people. I much regret that I have not been able to determine their specific gravity, but the experiments which can be made for the procuring of this result are very difficult, and, after all, do not furnish any real certainty; the transparency, however, of these crania alone proves this peculiar density. After what I have now said, it will not, I imagine, be difficult for anatomists to appreciate, by an attentive examination, the differences which we have now indicated.

This physical perfection of the bones of the head is equally apparent in all other parts of the skeleton. In fact, I have remarked that comparatively the bones of the limbs of the Arabs are more dense, and of a more compact tissue, without losing any thing of their elasticity; the eminences which afford insertion to the cords and the fibrous bands of the moving powers are very strongly marked; and this supplies these powers with so many very solid points of support, and so leads to the greatest precision of movement.

In addition, we have observed, 1*st*, That the circumvolutions of the brain, whose mass is in proportion to the capacity of the cranium, are more numerous, and the furrows which separate them are deeper, and the matter which forms the organ are more dense or firmer than in other races.* 2*d*, That the nervous system, proceeding from the medulla oblongata and the spinal cord, appears to us to be composed of nerves more dense in their structure than is found in Europeans generally. 3*d*, That the heart and the arterial vascular system present the greatest regularity, and a most perfect development. 4*th*, That the external senses of the Arabs are exquisitely acute and remarkably perfect; their sight has a most extensive range, they hear at very great distances, and also perceive the subtlest odours; this perfection is likewise very conspicuous in all the vital organs.

The muscular or locomotive system is strongly marked, and is conspicuously designed under the skin; its fibres are of a deep red colour, firm, and very elastic, explanatory of the power and agility of this people. This physical perfectability is very far from being found among the mixed nations of a part of Asia and of America, and especially among the northern nations of Europe. Upon the whole, I am convinced that the cradle of the human race is to be found in the country on which we have had occasion to dwell; and this conclusion would be more decidedly reached, if we could ascertain the specific gravity of the bones of the true Arabs; for this would assuredly appear greater, other things being equal, than with people of other nations, who undoubtedly do not possess in the same perfection the other normal properties on which we have dwelt; and this again leads to the conclusion that the Arab is the primitive race.

I have found among the Spaniards, more especially the Basques and Catalonians, a great analogy in physical and instinctive qualities with the Arabians, from whom, without doubt, the majority of the inhabitants of Spain and of our Pyrenees are descended; to whom I may add the inhabitants of Corsica, and of many other of the islands in the Mediterranean. The people, or individuals, of other countries of the earth, whose form of head and organic structure most approximate to the physical constitution of the genuine Arabians, are necessarily possessed of a proportionate perfectability in their sensitive functions and their intellectual faculties.

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* These phenomena have been observed in the brain of the celebrated poet Byron. We have given an account of his *necropsy* in the 5th vol. of our *Clinique Chirurgicale*.

We shall confine our remarks on the present occasion to the statement of these general ideas, which are the result of the researches and comparative observations I have had an opportunity of making among many different nations in the four quarters of the globe. I flatter myself that they will be useful to the scientific commission which is appointed to make observations around Algiers, and the ancient kingdom of Syphax: they may also assist in suggesting rules of health for the preservation and propagation of the physical and instinctive qualities of this race of primitive men.

M. Larrey presented to the Academy the Corsican head, and the two others, to which he referred, that, viz. of an Arab, and of a young Parisian aged twelve years, also the head of a Negro. All these he presented to the Museum of Natural History.

On the Revolutions of Iceland, Political and Natural.[*]

ICELAND is perhaps the most singular country in the world. It is generally agreed that it received its first settlers from Norway. For a century or two, new colonies were seen successively to arise, which were nearly all headed either by bold adventurers, or by great Norwegian chieftains, who, persecuted by their kings, expatriated themselves that they might avoid tyranny. On landing in Iceland, they beheld before them open and unoccupied lands, on which they freely established themselves and their followers. They ere long induced some of the principal families of Sweden to join them. These noble families brought along with them wealth, information, and withal a certain degree of inquietude as it regarded the future. The main cause of this was the apprehended vengeance of the monarchs they had left, and, to avoid surprise from them, they entertained, in the different courts of Europe, confidential advisers, who might observe and report to them whatever intrigues or political movements occurred. The written communications of these agents were preserved in the annals of the island, in which were likewise inserted all important internal and domestic events. Hence resulted in the course of time a great collection of memoirs, the copies of which were multiplied, and circulating freely, and thus was there gradually spread over the whole mass of the population a taste for historical studies, which, in spite of occasional alternations of activity and languor, has been perpetuated even to the present day. At the present moment the inhabitants of Iceland are passionately devoted to the study of their own history, and to that of nations removed to the greatest distance from them; so that almost under our eyes, and in ages which we may designate modern, the Icelanders have followed the example set them by the most ancient Egyptians, as may be seen in the *Critics* and *Timæus* of Plato. It would even appear that the love of letters, which supposes, and induces, this love of serious studies, was common to the people of the northern nations; for we learn from Thomas Bartholin that, about the year 1450, a royal academy was formed at Copenhagen, which, it will be observed, is nearly two centuries previous to the institution of the most celebrated academies of Europe. But be this as it may, the Icelandic SAGAS (the name attached to these very early memoirs, in the Scandinavian tongue), have long enjoyed the highest celebrity. Besides several poems, and some cosmogonies, which are sufficiently absurd, there may be found in them the original traces of the fundamental laws of several contemporaneous people, and more especially of the English. And thus it is probable that if the Sagas had been investigated for other objects than the acquisition of antiquarian lore,—if they had been studied with a view to medical information, they would have disclosed many traditionary statements highly valuable for the science; for there is no people so barbarous as altogether to neglect one of the most important of its concerns, viz. that of its preservation; and hence the Icelanders, so peculiarly careful in every thing which bears on themselves, had the strongest inducements to attempt to transmit to posterity the recital of their own misfortunes, and the record of the means by which they were delivered from them. This subject we recommend to particular attention. As it happens the Sagas are now chiefly dispersed among the libraries of England, Denmark, and Sweden, where they whet the curiosity of Europeans. They have been removed from their natural resting-place, like the obelisks of Egypt. Additional ones, however, may still remain in Iceland, an island, which, at so early a period, wrote its own history, and printed its own productions, an island which has produced great lawyers, and historians, and which, in our own days, has its merchants and travellers who traverse the wide world, and study the laws and political relations of its several nations; an island, in which education is so generally spread that the children of both sexes read and write at the age of eight and ten years, and recite likewise the sublime writings of Greece and Italy; an island, finally, which still reckons amongst its natives men who, profoundly versed in contemporaneous literature, are not less familiar with that of other days.

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[*] From the *Voyage en Islande et au Groënland, exécuté sur La Recherche*, tome i. part i. Par *M. P. Gaimard*. Paris 1838.

We shall finish in a few words the political history of these people. Iceland being twice the size of Sicily, and the original establishments being far asunder, and not numerous, they at first maintained themselves separately, and in the enjoyment of the most perfect liberty. With time they became both more extended and more numerous, and this twofold progress having approximated them to each other, their relative bearings necessarily became more close and pressing. A union of their strength would have made them powerful and formidable; but discord divided and weakened them. Pride and all the evils it engenders, umbrage, jealousy, animosity, the rivalry of power, the hatred and vengeance of families, armed them the one against the other, and thus with their own hands they opened to tyranny that door which before they had been so careful to foreclose. Favoured by these disputes, which they were careful secretly to nourish, the kings of Norway invaded them, and established themselves as the sovereigns of the island. Thus anarchy was the cause of the loss of independence; and Iceland experienced the fate of Greenland, and even of Norway itself, which was subordinated to the sceptre of Denmark.

To these political revolutions, so destructive of all prosperity, were added the still more awful revolutions of nature. Volcanos were kindled in various parts of the island. They blazed forth sometimes singly, sometimes together; and then was effected, in the interior, and upon the surface of this devoted spot, agitated to its very centre, a universal convulsion. Rivers changed their courses, mountains their places. Yawning rents appeared every where, now beneath the beds of rivers, and now below the waves of the ocean. From these there issued gases, suffocating and deleterious; either inflammable, which blazed over the extent of many leagues; or dense white columns of steam, which were instantaneously projected to the height of 80, 100, and even 200 feet, carrying along with them great black rocks, which fell down again with a fearful crash. Islands were upraised in the middle of the ocean, like the island of Graham, and, like it, these islands again sank in the abyss. The forests were overturned; those beautiful forests, so much boasted of in the Sagas, were laid prostrate, and buried in morasses, whence they may now be obtained as great fossil trees. The cultivated lands were covered over with ashes and lava; and great farms and whole villages were overwhelmed and engulfed, with all their cattle and inhabitants. Whatever was spared by these fearful fires, and these subterranean watery eruptions, other agents destroyed. Furious hurricanes, great avalanches, vast slips or falling of mountains, with dreadful thunder-storms, together with pestilences among men, and murrains among cattle, including the eastern plague, which, in the year 1348, invaded and ravaged the whole world, nearly completed its destruction. Finally, to finish the melancholy picture, enormous rocks of floating ice, those great glaciers which cover and obstruct the Northern Ocean, which ever threaten the frail barks of man, whose resolution, notwithstanding, dares to brave them, those huge masses, carried along by the currents, find a barrier upon the northern coast of Iceland. These are sometimes crowded with the great white bear, which, at considerable risk, must be encountered and destroyed; and sometimes carry along with them great quantities of wood, which suggest the idea of moving forests; a singular sight, which instantly sets the imagination at work as to the origin of so strange a phenomenon. Most frequently these great trees appear along with the glaciers, connected with them, but intertwined among themselves; and they are sometimes so compressed and fretted the one against the other, that they actually take fire, and then exhibit the extraordinary spectacle of a devouring flame in the midst of the sparkling ice. Whence proceeds all this wood?—and in what region is it produced? Some of it carries its fruit along with it, and is thus recognised to be the product of Mexico; other portions of it come from Carolina and Virginia; others have descended the Mississippi, or have been conveyed to the ocean by the St Lawrence, while still other portions seem to have drifted from the Pacific, and, finally, from the northern shores of Siberia. By what opposing powers have they all been forced to this spot as a rendezvous? Is it the shock of adverse currents which has produced all the agitation? And have these agitations forced this wood from its original direction? Again, are these currents constant or successive? Are they promoted by, or in opposition to the prevailing winds? Regarding these trees, it is not to be denied that they are a great advantage to Iceland, for they supply all the wood which it contains. And as to the glaciers, they alone are sufficient to maintain in sterility a country which has been otherwise so exceedingly impoverished; and of all its grievous calamities, this is, beyond doubt, the most frightful. It is, in truth, clearly demonstrated, that if the culture of corn, the *cerealia* generally, is no longer, as formerly, practicable in Iceland;—if vegetation is both rare and insignificant;—if, instead of magnificent forests, this island can now support only a few sterile and mongrel herbs; if, consequently, it is deprived of fruit, and even of vegetables, it is owing to the cold which extends from these masses of congelation far over the land, keeping fast bound all germination, and extinguishing all vitality. In the years 1753 and 1754, this cold was so extreme, that it killed all the horses, and even the sheep, though in every way protected.

But notwithstanding, in the midst of so many causes of ruin, and in spite of such enormous losses in his wealth and numbers, the Icelander still maintains himself; because his pastures, the meat, and milk, and wool of his flocks,—the aquatic birds, and the birds of passage, which his gun procures him,—and, most of all, the living treasures of the waters with which every season crowds his rivers, lakes, and the surrounding ocean, supply him with superabounding sources of subsistence, which appear altogether inexhaustible. We speak not here of a few succulent plants which he may associate with these staples of support, augmenting their quantity, and modifying their effects. But we cannot omit to state, that if Iceland is crowded with mountains covered with glaciers, and if these glaciers seem to rise higher, and extend wider from year to year, the soil which forms it, on the other hand, reposes on a vast storehouse of subterranean fire, which everywhere forces itself to the surface, and somewhat elevates itself every year above the level of the sea, bringing hot springs to the surface, simple, salt, and sulphureous, whose temperature is so high that they readily boil meat, and even as it were dissolve it, in a few minutes. Now, it sometimes happens that this heat, in certain districts, is the companion of mountain streams, and the moisture thus added to the heat, clothes the surface of these favoured regions with a rich, savoury, and in some respects a perpetual, vegetation,—a truly astonishing phenomenon in so high a latitude. Whence it may have happened, that, if the antique elephant which some thirty years ago was so unexpectedly discovered in a great glacier in the northern coast of Siberia, may be considered as a representative of the organization which preceded the appearance of man on the globe, it would be only natural also to consider these portions of favoured land in Iceland, as a representation of those vast continents which were covered with forests, and consequently always endowed with heat, which were the dwelling places of the innumerable legions of those stupendous animals. But be this as it may, such is the striking contrast which travellers here perceive. At first view, Iceland, naked, scraggy, sad, like the cloudy sky which covers it, and whitened only by the cold sheets of snow and ice which envelope it,—without verdure, without vegetation, where the eye scarcely discovers a few thinly scattered habitations, sickens the heart: he hesitates to visit it, and would almost fear to descend into that deep and mournful solitude, where he imagines that all is suffering, and which is barely habitable. But we have only to familiarize ourselves a little with the person of the Icelander,—with his manners, his habits, his industry, and all the details of his domestic life, to recover from these first impressions,—to be charmed with a life so simple, with an ease and quiet which elevates him incomparably above the Greenlander, and even above the indigent population of Europe.

On the Colour of the Ocean. By M. Arago.

The study of the colour of the ocean has exercised the sagacity of a great number of philosophers and seamen, without our being able to affirm that the problem regarding it has hitherto been entirely resolved.

To the question, What is the colour of the sea? the responses, indeed, might be very nearly identical. It is, in fact, to an *ultramarine blue* that Mr Scoresby compares the general tint of the polar seas; it is to a perfectly transparent solution of *the most beautiful indigo*, or to *celestial blue*, that M. Costaz assimilates the colour of the waters of the Mediterranean; it is by the words *bright azure* that Captain Tuckey characterizes the waves of the Atlantic in equinoctial regions; it is also *bright blue* that Sir Humphrey Davy assigns as the hue reflected by pure water procured by the melting of snows and ice. Celestial blue, then, more or less deep, that is to say, mixed with smaller or greater quantities of white light, would appear to have been always the peculiar tint of the ocean. Is there any deception in this?

We shall speak chiefly of pure water: but the waters of the sea are often impregnated with foreign matter. Thus, for example, the green bands so widely extended, and so peculiarly striking in the polar regions, contain myriads of medusæ of a yellowish tint, which, when mixed with the blue colour of the water, produce the green. Near to Cape Palmas, upon the coast of Guinea, Captain Tuckey's vessel appeared to move in milk; which appearance arose also from multitudes of animals floating upon the surface, and so hiding the natural colour of the liquid. The zones of carmine red, which several voyagers have traversed on the great ocean, arise from the same cause. In Switzerland, according to Sir H. Davy, when the tint of the lake passes from blue to green, it is because its waters are impregnated with vegetable substances. Finally, near the mouths of great rivers, the sea has often a brownish hue, arising from the mud and other terrestrial matters, which are there held in suspension. We have thought it right thus to insist upon the colours produced by foreign matter mixed with the water, so that they may in no degree be confounded with those upon which we are yet to dwell.

The celestial blue tint of the sea is modified, and sometimes even totally changed, in those localities where the water is not very deep. It is, then, because the light *reflected* by the bottom reaches the eye mixed with the natural light of the water. The effect of this superposition may be calculated by the laws of optics; but we must join to our acquaintance with the nature of the two commingled tints, that, which is more difficult to ascertain, of their comparative intensities. Thus, a bottom of yellow sand but lightly reflecting, gives to the sea a *green* tint, because the yellow mixed with the blue, as every one knows, produces a green; now, without changing the shades, if you replace the dull yellow with a bright yellow, the slight blue of pure water will scarcely make this a lively light green, and the sea will appear *yellow*. In the Bay of Loango, the waters are always *deep red;* so much so, that it is said they are mixed with blood: and Captain Tuckey satisfied himself that the bottom is intensely red. Let us substitute for this bright red bottom, one of the same shade, but obscure and slightly reflecting, and the waters of the Bay of Loango would then appear *orange-coloured*, or even perhaps yellow.

Against this method of regarding the subject, one objection is made which, at first glance, appears serious: a bottom of white sand, it is said, ought not to alter the hue of the sea, for if white enfeebles the colours with which it mixes, at all events it does not change the tint. But there is a ready answer to this objection. For how can we be certain that the sand at the bottom is white? It is not in the open day, after we have fished up a portion, and so exposed it to the *white* light of the sun, or the clouds! Is the sand in the same condition when beneath the water? If in the open air you were to illuminate it with red, green, or blue light, it would then appear red, green, or blue. We have then still to inquire what colour strikes it at the bottom of the water?

Water is in the condition of many other bodies, which philosophers, chemists, and mineralogists, have very deeply studied, and which possess two kinds of colours,—a certain colour which is transmitted, and another colour, quite different from the first, which is reflected. Water appears of a blue colour, by reflection; and some imagine it is of a green colour by transmission. Thus, water disperses in all directions, after having *blued* it, a portion of the white light which went to illuminate it; this dispersed light constitutes *the proper colour* of liquids. As to the other rays, *irregularly transmitted*, their passage across the water makes them green, and this intensely in proportion as the traversed mass is thick.

These notions being admitted, we may now return to the case of a not very deep sea, with a bottom of white sand. This sand receives the light only through a stratum of water; the light, then, is green when it strikes the bottom, and it is with this tint it is reflected; and in the second traject which the luminous rays make through the same liquid in returning from the sand to the open air, their green tint sometimes so predominates, that it prevails over the blue. This, then, perhaps may be the whole secret which, to the practical seaman, is, in the time of calm, the certain and invaluable index of great depths.

We have put it, *in time of calm*, and not without reason; for when the ocean is agitated, the waves suitably elevated may, in fact, convey to the eye so large a quantity of *transmitted or green rays*, that the reflected blue ones shall be entirely masked. A few short observations will make this evident.

Let us imagine a triangular prism placed in the open air, horizontally, before an observer, somewhat lower than he is. This prism cannot conduct to the eye, in the way of refraction, any ray coming directly from the atmosphere. On the contrary, the anterior face of the prism will throw towards the observer a reflected atmospheric pencil, a great part of which, it is true, will pass above his head. This portion would require to be bent in its course, to be inflected, to be refracted from above downwards to reach the eye. A second prism, placed like the former, but nearer to the observer, would precisely produce this effect.

After these few words, every one, without doubt, has already made the assimilation which must lead to the conclusion towards which we are tending. The waves of the ocean are a kind of prism; no wave is ever solitary; the continuous waves advance nearly in parallel directions. Well, then, when two waves approach a vessel, a portion of the light which the anterior face of the second wave reflects, *traverses the first*, is there refracted from above downwards, and thus arrives at the observer placed upon the deck. Again, then, we see transmitted light, light which is consequently *made green*, reach the eye at the same time with the common blue tints; but these are the phenomena of great depths over white sand produced without deep water, and the green colour of the sea arises from the predominance of the transmitted colour over the reflected colour.

We have now thus hastily traced the imperfect lineaments of a theory of the colours of the ocean, that thereby voyagers may be directed in the studies they may have occasion to make on the subject. The investigation of circumstances which may oppose this theory will suggest to them experiments, or at least observations, which without it they probably would not have thought of. For example, every one will understand that the *prism waves* should not produce identical effects, whatever may be the direction of their propagation, and they will be led to expect some variation in the hue of the sea upon a change of the wind. Upon the lakes of Switzerland this phenomena is apparent; is it so also on the wide ocean?

Some persons persist in assigning an important influence to the blue of the atmosphere in the production of the blue of the ocean. It appears to us that this idea may be subjected to a decisive proof, and in the following manner. The blue rays of the atmosphere do not return from the water to the eye till after they have been regularly reflected. If the angle of reflection equals 37°, they are polarized. A piece of tourmaline will then completely eliminate them, and thus the blue of the sea will be seen apart without any extraneous mixture.

With the intention of being freed, as much as possible, from the influence of reflection when studying the colours of the ocean, some able navigators have recommended that we should always examine it through the trunk of the ship's rudder. In this method the water exhibits, in some points of view, beautiful *violet* tints; but with a little attention we may be convinced that these hues are not at all real; that they are only the effect of contrast; that they proceed from atmospheric light feebly reflected in an almost perpendicular direction, and coloured by their approximation to the transmitted green colours which almost invariably surround the rudder.

If there be some who wish to admit, and develope, this attempt at an explanation of the colours of the sea, which we have just given, or if there be others who wish to refute it, and substitute a better in its place, it will still be necessary to begin by investigating the colour of water when seen by transmission *with the aid of diffused light.* Those who will recall to recollection the pre-eminently green hue of the cut edge of a crystal glass, even when this glass is only illuminated in front and perpendicularly, will perceive all the importance of this remark. The following appears a very simple means of reaching a satisfactory conclusion.

I shall suppose that the observer is supplied with one of those large hollow ice prisms, which philosophers are in the habit of using when they wish to study the refraction of liquids. To make our apprehensions the more precise, we shall make the refracting angle equal to 45°, we shall then suppose that the prism is plunged *partially* into water, so that the edge of its refracting angle shall be downwards and horizontal, and that one of the faces of this angle, viz. that which is most exposed (est tournée vers le large), shall be vertical, whence it will result as a necessary consequence that the other face will be inclined to the horizon at an angle of 45°.

When the objects are thus arranged, the light which moves horizontally in the water at a fraction of an inch below the surface, that which constitutes the *colour of its cutting edge* (*Sa colour de tranche*), if I may be permitted to use this expression, will strike perpendicularly the vertical ice of the prism; it will penetrate into the interior of this instrument, will traverse the small quantity of *air* which it encloses, will reach the second plate of ice, and will there be reflected vertically from below upwards. In looking upon this inclined ice, the observer then may judge of the proper colour which the water has by refraction, quite as well as if his eye were in the liquid. In this form, the experiment is so simple and so easy, it requires so little time that we shall venture to request the Academy to recommend to our voyagers to repeat it as often as possible, not only with sea-water, but also with that of lakes and rivers. When the science shall be enriched with the results of all these observations, we shall no longer run the risk of constructing theories which facts sooner or later will contradict.

There is assuredly no occasion to remark that it is desirable that the hollow prism should be enclosed at its upper part by a piece of ice enclosed in glass, and having parallel faces. This will prevent the apparatus from being filled with liquid. The instrument can easily receive the form of the common instrument from the hand of an artist.—*Comptes Rendus*, No. 4. July 25. 1838.

Description, with Drawings, of a Grinding Machine which is used instead of a Turning Lathe, for giving a truly Cylindrical *form to the rims of Pulleys and Drums :—also Drawing and Description of a Machine for Grinding Pulleys* round *on the rim.* By Mr James Whitelaw. (Communicated by the Society of Arts.) With two Plates.

Glasgow, 19*th January* 1838.

Gentlemen,—Mr Edward Sang, in his paper on the Progress of Exactitude in the Manufacture of Machines, has given you an account of the latest improvements made on the turning, planing, and screwing machines. But besides these machines there is the grindstone, which has been fitted up of late, so as to do certain kinds of work in an expeditious and accurate manner, and from some experience which I have had in working it, I am of opinion that it might be applied to a greater variety of work, and much more extensively than it has ever yet been. I will *first* give you an account of a machine planned by me for grinding pulleys or drums *truly cylindrical* on the rim, which has been much in use during the last eighteen months. After I have described this first machine, I will give you an idea of *another* machine, which has never yet been brought into practice, for grinding pulleys, &c. *round* on the rim; and then you will, I believe, be of opinion that grinding machines might be used to great advantage in factories where mill-gearing is made.

Plate I. Fig. 1, of sketch No. 1, is a side elevation of a machine for grinding drums or pulleys of a truly cylindrical shape, or, as it is called, straight on the rim, and Fig. 2, is a ground-plan of the same machine. The parts which are seen in both Figs. are marked by the same letters in the one Fig. as in the other; *aa* is the grindstone, and *bb* is the pulley which is to be ground. The pulley *b b* is fastened upon the mandril *c c*, this mandril is fixed into the spindle *d d* at the one end, and it works into a plummet-block *e* at the other end. The speed of the grindstone is 180 revolutions per minute, and the pulley *bb* makes 130 revolutions in the same time; the stone and the pulley revolve in the same direction, in order that the parts in contact may rub upon each other at the combined speed of their circumferences. There is a small shaft *fff*, which, by means of the mitre wheels fixed upon it, gives motion to the screws *gg*, *gg*, *gg*, in connection with the plummet blocks, into which the spindle *d d* and the mandril *c c* work. As the plummet-blocks slide upon the rails *h*, *h*, *h*, which form part of the framing of the machine, in a similar manner as the parts of a slide-rest move, and as all the screws marked *gg* have the same pitch of thread, the spindle and the mandril are shifted at once, either from, or towards the grindstone, and keep always parallel to its shaft, if the handle or wheel *v* is moved by hand. Upon the end of the shaft of the grindstone, a bevel pinion *i* is fixed, and this pinion gears into the wheel *k k*, fixed upon the shaft *l*. The shaft *l* is bored out to fit the shaft *m m*, which is turned parallel

the whole of its length, so as to slide through the shaft *l* easily. There is a feather fixed into the shaft *l*, which fits the groove planed into the shaft *m m*, so that this shaft can slide through the shaft *l*; but it cannot revolve unless the shaft *l* carry it round. Upon the end of the shaft *m m* a small crank *n* is fixed; as this crank has a perpendicular position in the Figs. it is not so distinctly seen. The crank pin has a brass on it, which slides into a groove running in a perpendicular direction in the part *o o*. As two sets of the bushes in the part *o o* work upon journals cut into the spindle *d d*, if the grindstone revolve, the spindle *d d* and the mandril *c c* will work endways, at one time in one direction, then in another, so as to bring an end of the pulley *b b* past a side of the stone, once for every half revolution of the crank shaft; and, in this way, the pulley and the stone, as they wear, keep perfectly cylindrical. It will be evident that the mandril and the spindle have no ruffs, and they are turned parallel at the parts which slide and revolve into the plummet-blocks. The bracket *p p*, which supports the crank-shaft, is fixed upon the two plummet-blocks for the spindle *d d*, and, on this account, if they are moved from or towards the grindstone, it is carried along with them. The pulley *q*, which drives the spindle *d d*, is connected to it (the spindle) in a similar way as the shaft *m m* is connected to the shaft *l*, and the part of the bracket *p p*, seen in the ground plan passing nearly round this pulley, keeps it always at its proper place. The small rod *r r* serves to guide the part *o o*, and keep its groove always in a perpendicular direction. *S* is the pulley which drives the grindstone.

The machine now described grinds fifteen pulleys of 18 inches diameter by 5 inches broad, in a day, working ten hours; now this is a great quantity of work, considering the very moderate speed at which the parts are driven. When a pulley is finished, if a planed malleable iron straightedge is applied on the rim parallel to its axis, it touches the pulley in every part. After a pulley is ground, it is shifted back from the stone and still kept revolving, then a stick with a little emery and oil on it is pressed against the pulley, and this gives it a fine polish. A grindstone for finishing pulleys is not anything like so expensive to make as a turning lathe: it does incomparably more work, and it does it as well as any self-acting lathe; besides, when the iron is hard, which is frequently the case in thin pulleys, a lathe is good for nothing, and this makes no difference in a grinding machine: a grinding machine will finish pulleys cast much thinner than any that can be turned; this is a saving of metal: from the manner in which the ends of a pulley work past the sides of the stone, it always keeps in good order: all these are advantages which a grinding machine has over a lathe, in finishing pulleys or drums.

As the crank is made forked, and as the part *o o* has a groove in it open at both ends, for the bush of the crank-pin to work into, the crank-pin can easily be taken away by unscrewing the nut which holds it to the crank, and then the spindle *d d* will not be wrought endways by the motion of the crank. When the ends of a pulley are to be ground, the crank-pin is taken away and a pincing screw is fixed upon the rod *r r*, which acts upon the part *o o*, so as to press the spindle *d d* on end, the one way or the other, as required; then if the pulley is shifted past the side of the stone, the pincing screw will press its end against the stone, and, if the machine is kept in motion, the end of the pulley will be ground. Having the crank forked answers another purpose; it allows the pin to be shifted further from, or nearer to, the centre of its shaft, in order to give a longer or shorter range to the motions which the crank gives to the spindle and mandril. A machine for grinding drums should be made wider than the one shewn in the sketch, at the place where the stone and the pulley to be ground work, in order to get the ends of a long drum ground, when they are not finished in the turning lathe, at the time the drum is chucked, to get its eye bored out. Perhaps the simplest way of finishing the ends of a pulley which is bored out in the eye, is to do it in the lathe when it is on the chuck. When a pulley is finished, and it is to be taken out of the machine, or when a rough pulley is to be put into the machine, the cutter *t* is driven out, and then the mandril *c c* is drawn on end by means of the handle *u*, after the key which holds the pulley upon the mandril is slackened. Any of the plummet-blocks which guide the spindle *d d* and mandril *c c* can readily be taken away, and a larger or smaller one put in its place; by this means a spindle or mandril to suit any sort of job may be used. The stone is covered in by means of a wooden box, so that the water used in grinding may not be thrown about the workshop. The scale in Plate II. gives the dimensions of the parts of the machines shewn in both plates.

The machine shewn in Figs. 1, 2, and 3 of Plate II. is one for grinding pulleys round on the rim; in all the Figs. the same letters point out the same parts. *a a* is the grindstone; *b b* is the pulley to be ground; this pulley is fastened upon the mandril or spindle *c c*. When a pulley to be ground is put upon the mandril *c c*, or when a finished pulley is to be taken off this mandril, the pincing screw *d* is slackened, and then the ruff into which it is screwed gets loose upon the mandril; after this the mandril is drawn on end, so far as that a pulley may either be taken off, or put upon it, by taking hold of the pulley *e* which gives motion to the mandril. The two screws *f f*, *f f* work into nuts fixed into the bracket *g g g g*, and by moving the handle or wheel *h*, the pulley to be ground is pressed against the stone, or shifted away from it as is wanted. The bracket *g g g g* has two pins or gudgeons *i i* fixed on it, and the frame *l l m m* turns upon these pins as a centre. When a pulley is to be ground very flat on the rim, the plummet-blocks *k k* are shifted as close to the rail marked *m m* as they can get, by turning the handle *n*. The closer that the plummet-blocks, into which the mandril *c c* works, are brought to the rail *l l*, the pulley will be ground with the more curvature on its rim. The shaft *o o* is set in motion, by means of the bevel wheels *p* and *q*; this shaft has an eccentric *r* upon it, which, by means of the connecting rod *s*, gives motion to the frame *l l m m*. By slackening the pincing screw *t*, the eccentric may be shifted along the shaft *o o*, and by slackening the pincing screws *u* and *v*, it may be shifted along the rod *w w*, so as to give as much travel to the frame *l l m m* as is required. As the frame *l l m m* is always in motion, it might perhaps be better to have a guide round the pulley *e*, in order to keep the belt on it. After the plummet-blocks *k k* are brought to the place on the rails of the frame *l l m m*, which gives the pulley its required curvature, the machine is set in motion, and the pulley is pressed against the stone by means of the handle *h*.

In order to get a very large pulley put into, or taken out of the machine, the rail *x* must be taken away, by unscrewing the bolts which fix its ends. In the bracket *g g g g*, each of the parts which hold the pins *i i* has a slit in it, so that, by unscrewing the nuts *y y*, the pins may be shifted to any end of the slits. One of these slits is seen in Fig. 2; it has a check round it to receive a shoulder formed upon the pin. In the frame *l l m m* each of the parts which hold the bushes for the pins *i i*, has a slit formed, so that the bushes can be fixed on either end of it; there are cutter holes placed so that the bushes can have other positions, besides at the ends of the slits. When the bushes are not fixed at any of the ends of the frame *l l m m*, a gib, as shewn in Fig. 2, is passed through a set of cutter holes; after this the brasses are brought up against it, and fixed by means of the same cutter which fixes them at the ends of the slits. By having the slits for the pins in the bracket *g g g g*, and the slits for the bushes in the frame *l l m m*, allows the rails upon which the bracket *g g g g* slides, as well as other parts of the machine, to be made much shorter than they could be made without them, and some sizes of pulleys could not be ground at all without these slits. The use of the slits in the bracket *g g g g* is to allow the mandril *c c* in every position to work clear of the part of this bracket, which has an upright direction in Fig. 1. The cutter holes in the frame *l l m m* must be fitted, so that, in whatever place in the slits the bushes are shifted to, the rails *l l* and *m m* will have an upright position, in a view, as per Fig. 1; and when the pins *i i* are shifted to the different ends of the slits, in the bracket *g g g g*, from what they are shewn in the Figs., these rails of the frame *l l m m* must have an upright position, in a view, as per the side elevation. It will be seen that Fig. 3 is not a complete end view, but is only intended to shew some of the parts.

If the smallest speed of a cone was made in the form of a pulley, with a flanch cast inside of its rim close to one end; and then, if the next larger speed was cast without arms, but having two flanches, one at each end of its rim, the parts now spoken of could be fixed together so as to form a cone of two speeds, by means of bolts passing through the flanch on the small speed, and one of the flanches on the large speed. On the principle now explained, a third speed could be fixed upon the cone, then a fourth, and so on, till the required number of speeds was put upon it. In a cone having a great many speeds, the largest speed should be cast with arms and a centre, the same as for the small speed. A cone of this construction could be ground upon a machine, as last described, by separating it into speeds, and grinding the smallest one first; then the second smallest speed could be ground after it was bolted upon the smallest speed, and so on, grinding one speed after another, till the cone is finished. If cones were to be finished upon a grinding machine, it would require to be made wider than the one to be shewn in sketch No. 2.*—I am, Gentlemen, yours respectfully, JAMES WHITELAW,

Late of 18 Russell Street, Glasgow,
now in London.

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* REPORT on Mr WHITELAW's Machine for grinding Cast-Iron Pulleys.

EDINBURGH, 18*th July* 1838.

Your Committee, having attentively considered the drawing and description of this machine, are of opinion that it will produce the required effect. They imagine, however, that there is a considerable defect in the arrangement of the relative positions of the grindstone and of the drum or pulley to be ground. The communication is one of very high merit, and relates to a subject every day becoming more important; the Committee, therefore, consider it worth while to criticise minutely the point referred to.

In Mr Whitelaw's arrangement the axes of the drum and of the grindstone are parallel to each other. Two inconveniences result from this:—In the first place there is created a tendency in the drum to follow the motion of the grinder, which causes an undue, though not very considerable, strain on the band or belt which leads the drum. In the next place, there is a tendency to streak the surface. It is true that arrangements are made to shift the drum length-ways upon the stone, so as to equalize the wear, and to shift the positions of the streaks, but this, in the opinion of your Committee, will merely palliate, not effectually correct the evil. If the axes were placed at right angles to each other, both of these evils would be removed; and if, in addition, the motion of one or other shaft were reversed at intervals, a surface would be produced superior both in general accuracy and in finish. The streaks then would cross each other obliquely on the drum, and the ultimate result would be a very *flat* surface; of this, one of your Committee has had abundant experience.

Mr Whitelaw's proposed method of rounding pulleys would, no doubt, answer perfectly; indeed, there appears to be no distinction in principle, and as little in practice, between the machine for giving a straight, and

On Storms. By Mr WILLIAM C. REDFIELD of New York, and Lieutenant-Colonel REID, Royal Engineers.

COLONEL CAPPER of the East India Company's service, in a work on Winds and Monsoons, published in the year 1801, states it as his opinion, that hurricanes will be found to be great whirlwinds; and says, "It would not, perhaps, be a matter of great difficulty to ascertain the situation of a ship in a whirlwind, by observing the strength or changes of the wind. If the changes are *sudden*, and the wind violent, in all probability the ship must be near the centre of the vortex of the whirlwind; whereas, if the wind blows a great length of time from the same point, and the changes are gradual, it may reasonably be supposed that the ship is near the extremity of it."

This view of the nature of hurricanes appears to have been lost sight of for a long time, or to have been mentioned only in a very cursory manner, until an American observer, Mr W. C. Redfield, published in the 20th volume of Silliman's well known American Journal of Science and Arts, a valuable memoir, entitled "*Remarks on the prevailing Storms of the Atlantic coast of North America*," in which he maintains (and we believe without any knowledge of Capper's work) that these storms are *great whirlwinds.* This memoir, inserted in the 18th vol. of *the Edinburgh New Philosophical Journal*, from its important details, and the general plausibility of the explanation offered, we esteemed a valuable contribution to the natural history of the winds. In the year 1834 we were again gratified by receiving from Mr Redfield a copy of another memoir, entitled "*Observations on the Hurricanes and Storms of the West In-*

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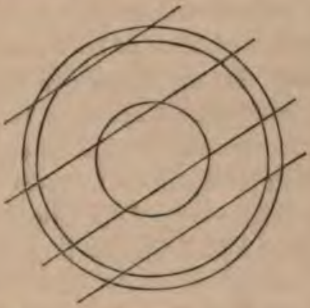
that for giving a circular outline; here, also, the crossing of the two motions ought to be attended to; were that done, there would probably be no use found for oil and emery afterwards.

On the whole, your Committee beg to recommend this interesting communication to the attentive consideration, and favourable notice, of the Society.

EDWARD SANG, *Convener.*
WALTER NICOL.

dies and the coast of the United States, with a chart," in which his opinion, as to the nature of storms, is farther enforced and supported by numerous additional observations. This memoir and the accompanying chart were also published in the 20th volume of *The Edinburgh New Philosophical Journal.* In this way we enabled British meteorologists to become acquainted with Mr Redfield's observations and views. As our meteorologists generally had taken but little notice of these memoirs, we were rejoiced to find them brought prominently before the British Association, at Newcastle, by a very intelligent officer and excellent observer, Lieut.-Col. Reid of the Royal Engineers, in a "*Report explaining the progress made towards developing the Law of Storms, and a statement of what seems desirable should be further done to advance our knowledge of the subject.*" Colonel Reid, at the meeting of the Physical Section of the British Association, commenced by stating "that he had long been convinced that the operations of the Deity in the workings of his providential care over his creatures, were governed by fixed laws, designed by incomprehensible wisdom, arranged by supreme power, and tending to the most benevolent ends. That however irregular the tempest or the tornado might appear to the inobservant, yet our own day had seen some of these phenomena reduced to rule; and he doubted not soon to convince the Section that we were on the eve of advancing some steps farther towards this most desirable end. He felt confident, indeed, that the laws of atmospheric changes were dependent on such fixed principles, that nothing was wanting but a more intimate acquaintance with the subject, to render man's knowledge of these laws as perfect as that which he had attained in any of the sciences now called strict. His attention had been first directed to the subject in 1831. He arrived, on military service, at Barbadoes, immediately after the desolating hurricane of that year, which, in the short space of seven hours, destroyed 1477 persons on that island alone. He had been for two years and a half daily employed as an engineer officer amidst the ruined buildings, and was thus naturally led to the consideration of the phenomena of hurricanes. The first explanation which to him seemed reasonable, he found in a pamphlet by W. C. Redfield of New York, extracted from the American Journal of Science, a work much less known in this country than its value and great merits deserved. The north-east storms on the coast of America had attracted the attention of Franklin. He had been prevented by one of these storms from observing an eclipse of the moon at Philadelphia, which he was soon after astonished to find had been felt at Boston, although that town lay to the north-east of Philadelphia. This was a circumstance not to be lost on such an inquiring mind as Franklin's: he ascertained, upon inquiry, that the same north-east storm had not reached Boston for some hours after it had blown at Philadelphia; and that, although the wind blew from the north-east, yet the progress of the entire storm was from the south-west. He died, however, before he had made any further progress in this investigation. Col. Capper of the East India Company's service, after having studied meteorological subjects for twenty years in the Madras territory, published a work, in 1801, upon winds and monsoons, giving brief statements of their fatal effects, from Orme's 'History of Hindostan.' In this work he states his belief that hurricanes will be found to be great whirlwinds; and says, 'it would not, perhaps, be a matter of great difficulty to ascertain the situation of a ship in a whirlwind, by observing the strength and changes of the wind. If the changes are *sudden*, and the wind violent, in all probability the ship must be near the centre of the vortex of the whirlwind; whereas, if the wind blows a great length of time from the same point, and the changes are gradual, it may reasonably be supposed that the ship is near the extremity of it.' In this conjecture respecting the nature of hurricanes, Col. Reid conceived Col. Capper to be decidedly right, and the conclusion he drew from it has stood the test of close examination. Mr Redfield, following up the observation of Franklin, and though probably unacquainted with the views or opinions of Capper, ascertained that, while the north-east storms were blowing on the shores of America, the wind was with equal violence blowing a south-west storm on the Atlantic. Tracking Franklin's storms from the southward, he found, throughout their course, that the wind on opposite sides of the shore, over which the storm prevailed, blew in opposite directions, and that, in fact, the entire storm was a progressive whirlwind, and that all these whirlwinds revolved constantly in the same direction. In one of the numbers of the American Journal of Science (for 1831), Colonel Reid found collected together by Mr Redfield many records of the same storms, and a chart, on a very small scale, shewing the progress of one. Strongly impressed with the conviction that Mr Redfield's views were correct, he determined to verify them by making charts on a large scale, and laying down on them the different reports of the directions of the wind at points given in the American Journal of Science; and the more exactly this was done, the nearer was the approximation to the tracks of a progressive whirlwind. He then exhibited to the Section a volume containing eight charts on a large scale, of which the first and second chart contained the result of this part of the examination; and he explained how the arrows shewing the direction of the wind at the several stations were all on the right hand side of the several circles flying from the south, while at the stations at the left hand, or towards the east of the chart, they were all coming from the north. Colonel Reid went on to explain, that as his object was not to establish or support any theory, but simply to arrange and record facts, he had only at present to give such a sketch of what had been done, as would turn the attention of abler men than himself to this investigation, and to impress upon commercial men the importance of carefully preserving the logs of their merchant ships: the practice was, he found, to return these logs to the brokers so soon as the vessel returned to her port, and after his accounts were balanced, they were considered as of no further value. He had published at length the details of his examination of this question. He had procured the actual log-books of ships, and had combined their information with what he could obtain on land, thus comparing simultaneous observations over extended tracts. On the eighth chart he pointed out eight ships in several positions in the same storm, the tracks of several crossing the path of the storm, and the wind, as reported by the ships, corroborated by the reports from the land. The observations of ships possess this great advantage for meteorological research, that merchant-ships' log-books report the weather every two hours, and ships of war have hourly observations always kept up. After tracing a variety of storms in north latitudes, and being impressed with the regularity with which they appear to pass toward the North Pole, and always revolved in the same direction,—viz. opposite to the hands of a watch, or from the east round by the north, west, south, and east,—he was led to conclude, that, in accordance with the order of nature, storms in south latitudes would be found to revolve in a contrary direction to that which they take in the northern hemispheres. He earnestly sought for facts, to ascertain if this were really the case, and had obtained much information confirmatory of the truth of the conjecture, before he was aware that Mr Redfield had formed the same conjecture, without, however, having traced any storms in south latitudes. The general phenomena of these storms will be understood, if the storm, as a great whirlwind, be represented by a circle, whose centre is made to progress along a curve, or part of a curve, which is, in most cases, of a form approaching the parabolic, the circles expanding as they advance from the point at which the storm begins to be felt, the rotatory motion in the northern hemisphere being in the contrary direction to that in which the hands of a watch go round; while, in the southern hemisphere, the rotation is in the same direction as that in which the hands of a watch revolve. He pointed out how his views were illustrated by the disastrous storm of 1809, experienced by the East India fleet, under the convoy of the *Culloden* line-of-battle ship, and the *Terpsichore* frigate, and four British men-of-war, which left the Cape of Good Hope about the same time, intending to cruise about the Mauritius. Some of these vessels scudded and ran in the storm for days; some, by lying to, got almost immediately out of it, while others, by taking a wrong direction, went into the heart of it, foundered, and were never heard of more: others, by sailing right across the calm space, met the same storm in different parts of its progress, and the wind blowing in opposite directions, and considered and spoke of it as two storms, which they encountered; while others, by cruising about within the bend of the curve, but beyond the circle of the great whirl, escaped the storm altogether, which had been for days raging on all sides of them. This led him to draw the very important practical conclusion as to how a ship should act when she encountered a gale, so as to escape from it. By watching the mode of veering of the wind, the portion of a storm into which a ship is falling may be ascertained: if the ship be then so manœuvred as that the wind shall veer aft instead of ahead, and the vessel is made to come up, instead of being allowed to break off, she will run out of the storm altogether; but, if the contrary course be taken, either through chance or ignorance, she goes right into the whirl, and runs a great risk of being suddenly taken aback, but most assuredly will meet the opposite wind in passing out through the whirl. To accomplish her object, he shewed, by a diagram, that it was necessary that the ship should be laid on opposite tacks, on opposite sides of a storm, as may be understood by drawing a number of concentric circles to represent the whirl of the hurricane, and then different lines across these, to represent the course of ships entering into, or going through the storm: but, to attempt the full explanation of even this, would extend much beyond our limits.



Colonel Reid illustrated his views by reference to various circumstances connected with the great hurricane of 1780, and the position of the several ships of Sir George Rodney's squadron, as also those of the East India convoys in the hurricanes of 1808 and 1809. He pointed out the effects of these storms on the barometer and sympiesometer, and the practical lessons to be derived from their indications. He highly eulogized the anemometers of Professor Whewell and Mr Follett Ossler, and particularly dwelt upon the importance of having the latter instrument placed at various stations in the usual tracks of these great hurricanes, as a means of deciding several important questions connected with them. He likewise pointed out the value of inducing the several maritime nations to establish registers at their light-houses, and mutually to communicate their observations, from which would result a fund of most valuable information, which would doubtless throw light on this, and on other collateral subjects. He pointed out the coincidences which existed between the revolving motions of storms in the two hemispheres, and those which galvanism caused around the poles of magnets; thus he saw the magnet, when in conjunction with the voltaic battery, making contrary revolutions around the two poles. He also stated, that where Major Sabine had found the magnetic intensity least, viz., at St Helena, there were no violent storms, his line of least intensity appearing to be the true Pacific Ocean of the world. The lines of greatest magnetic intensity, on the contrary, seemed to correspond with the localities of hurricanes and typhoons; for we find the meridian of the American magnetic pole passing not far from the Caribbean sea, and that of the Siberian pole through the China sea. He shewed that the phenomena of water-spouts were exactly the reverse of those of hurricanes, and alluded to their electrical states. He mentioned two instances of water-spouts, one in the northern the other in the southern hemisphere, in which the revolutions were in opposite directions, but both in the contrary direction to great storms. He explained the variable high winds of our latitudes, by the storms expanding in size and diminishing in force as they approach the poles, and the meridians at the same time nearing each other, occasioning a huddling together of the gales. He further remarked, because the diameters of these circles, over which the whirl of the storm was spread, often extended from 1000 to 1800 miles, observations made in the meteorological stations in the British isles, however valuable for other purposes, would not, by themselves, suffice for throwing light on this question.

The celebrated American philosopher, Professor Bache of Philadelphia, brought forward a rival, but unsatisfactory theory of storms—that proposed by the ingenious Mr Espy of Philadelphia. Sir John Herschel said he had received from Mr Redfield his papers on this subject, and embraced this opportunity of publicly expressing his thanks, and of stating the great pleasure he had derived from their perusal. It was not only at sea that the practical value of this splendid discovery respecting hurricanes would develope itself in enabling the sailor to escape its violence, instead of running ignorantly into the very jaws of destruction, by attempting to run away; but even on land it would suggest invaluable hints for the securing of life and property. One or two circumstances connected with Colonel Reid's charts particularly impressed him. The first was the curious parabolic shape of the curves denoting the progress of these storms, so well calculated to give unfailing directions as to the nature and course of a storm when accidentally encountered at sea, as the sailor had only to consider the parts of these curves in which he was placed, and the veering of the wind, and he had almost placed before him a chart of the hurricane. He next threw out the suggestion for Colonel Reid's consideration, whether the Gulf Stream would not perhaps give a clue to the direction of these curves, as so large a body of comparatively warm water must most materially tend to heat the air above it, and thus occasion disturbances of atmospheric equilibrium. Colonel Reid had stated that he had no theory: in this, no doubt, he was judicious as an observer; but, as in the present assembly, a theory, if it served no better purpose, helped memory, suggested views, and was even useful by affording matter for controversy, which might produce brilliant results by the very collision of intellect. In the second place, he remarked, that in the southern hemisphere the oscillations of the barometer, which were in an opposite direction to those of the northern, afforded a strong confirmation of the correctness of Colonel Reid's views. These revolving hurricanes reminded him, that on discharging a great gun unshotted, the mouth of which had been previously greased, a beautiful ring of smoke is formed, which passes to a considerable distance with much permanence, but enlarging constantly in diameter: upon attending closely to this, every part of the ring will be found to be in rapid revolving motion, thus exhibiting to the eye a hurricane in miniature, performing its evolutions. That water-spouts should deviate from the law of storms was to be expected. He supposed them to arise more from some local cause of disturbance, than from any great revolution in the currents of the atmosphere. Upon the land they might be produced by local circumstances, such as a heated space of ground, which would force the current upwards; and he could imagine water-spouts revolving in either one direction or the other. As to Mr Espy's theory, though he considered it ingenious, yet he did not see how it was tenable against the indications of the barometer.

On the probable Duration of Human Life.[*]

Dr Caspar of Berlin, in his valuable work, entitled 'Der wahrscheinliche Lebensdaur des Menschen, &c.,' 1835, after having examined the current opinions as to the average duration of human life, and as to the most satisfactory method of ascertaining such a result, announces his own doctrine in the following proposition:—*The proportion of births to the population in any place expresses almost exactly the medium or average duration of life there.*

For example, suppose that this proportion is in the ratio of 1 to 28, then the average life of the inhabitants of the place will be found to be 28 years.

If this rule be correct, it must follow that the duration of life increases and diminishes in a population, according as their fecundity is greater or less; so that man, if not as an individual, at least as a member of the mass, may be said to have it in his power to lengthen or to abridge his life.

This, if true, is indeed a proposition of great importance in political economy.

To prove that the mortality is in a direct ratio with the fecundity of any population, and, consequently, that governments, seeing that the force of states consists not so much on the mere number, as on the strength, fecundity, and longevity of their inhabitants, ought not to favour or encourage an over abundant population, the author has collected together a vast number of facts, and for this purpose has drawn up tables of the mortality, not only in Prussia, but also in Britain, France, and Belgium.

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[*] Medico-Chirurgical Review, July 1838.

From these researches he comes to the conclusion, that everywhere the mortality is directly proportional to the fecundity of the population.

This doctrine, if confirmed by future inquiries, may, to a certain extent, reconcile the opinions of Malthus and his opponents, as it shews us that Nature herself tends to remedy the evil of a redundant population.

Dr Caspar gives a valuable table of the mortality in Berlin, for twelve years, from 1817 to 1829, which comprises nearly 70,000 deaths in nearly 2,000,000 inhabitants.

The following are a few interesting data which are derivable from his researches.

The longevity of the female, is greater than that of the male sex.

The age of puberty carries off 8 per cent. more of the female than of the male sex.

The proportion of deaths of women in labour is 1 in 108.

It has been an erroneous, although hitherto a very prevalent notion, that the climacteric age of women has a marked influence in increasing the mortality of the female sex.

This opinion has been shewn to be incorrect by several statistical writers, and the researches of Dr Caspar confirm the accuracy of their statements. On the whole, therefore, we may assert that the longevity of the female is greater than that of the male sex.

It is also worthy of notice that of still-born infants, there are more of the male than of the female sex.

Dr Caspar proceeds to shew that the medium or average duration of life has increased considerably in most European cities of late years. In London this increase is great, for it would seem that, within the last century, probable life has increased by twenty years.

At Geneva, again, in the 16th century one-half of the infants born there died, we are told, before their fifth year; whereas, in the present day, it would appear that this half reaches nearly 43 years of age. A similar remark may be made as to the increased length of life at Berlin.

Dr Caspar treats pretty fully on the influence of pursuits and occupations on the duration of human life; and from his inquiries it appears that *clergymen are, on the whole, the longest, and medical men are the shortest livers.* The different classes may be arranged, in respect to longevity, as follows:—

	Medium Longevity.
Clergymen,	65 years.
Merchants,	62 do.
Clerks,	61 do.
Farmers,	61 do.
Military Men,	59 do.
Lawyers,	58 do.
Artists,	57 do.
Medical Men,	56 do.

Another important agent or influence on the probable duration of life is *marriage.* It is proved by the researches of our author that *the married state is favourable to longevity,* and especially in reference to the male sex.

The influence of poverty and destitution in shortening the medium duration of life is well known. Dr Caspar gives some tables of mortality which prove the sad contrast in this respect between the poor and the affluent. From these it would seem that the medium age of the nobility in Germany may be stated at about 50 years, whereas that of the paupers is as low as 32 years.

The last chapter of the work treats of the influence of the fecundity of a population upon its mortality. Dr Caspar shews, by a vast number of documents, that "*the mortality in any population is always in exact ratio to its fecundity,*" or, in other words, "*the more prolific the people is, the greater usually is the mortality among them.*"

He alludes to the difference, in this respect, in the different districts in England; and maintains that wherever the number of births is highest, there the mortality is greatest at the same time.

The same result is derivable from statistical investigations in Belgium, France, and other countries.

Dr Caspar concludes his work by embodying the general principles of his researches in the following conclusions:—

1. The proportion of births to the actual stationary population of any place expresses, or is relative to, the medium duration of life in that population.

2. The female sex enjoys, at every period of life, except at puberty, at which epoch the mortality is rather greater among young females, a greater longevity than the male sex.

3. Pregnancy and labour occasion, indeed, a considerable loss of life; but this loss disappears, or is lost, in the general mass.

4. The so-called climacteric periods of life do not seem to have any influence on the longevity of either sex.

5 The medium duration of life, at the present time, is in Russia about 21 years, in Prussia 29, in Switzerland 34, in France 36, in Belgium 36, and in England 38 years.

6. The medium duration of life has, in recent times, increased very greatly in most cities in Europe.

7. In reference to the influence of professions or occupations on life, it seems that ecclesiastics are, on the whole, the longest, and medical men are the shortest livers; military men are nearly between the two extremes, but yet, proportionally, they, more frequently than others, reach very advanced years.

8. The mortality is very generally greater in manufacturing than in agricultural districts.

9. Marriage is decidedly favourable to longevity.

10. The mortality among the poor is always greater than among the wealthier classes.

11. The mortality in a population appears to be always proportionate to its fecundity,—as the number of births increases so does the number of deaths at the same time.

Short Account of some Researches upon the Variations which take place at certain times of the day in the Temperature of the Lower Strata of the Atmosphere. By Professor MARCET.[*]

THE author first examines the observations which had previously been made upon this subject, and more particularly those of M. Pictet, and of the English naturalist Six, undertaken towards the close of the last century. After demonstrating that the observations of these philosophers are not sufficient in a satisfactory manner to resolve the different inquiries which present themselves, the author proceeds to the description of the apparatus which he himself employed in the prosecution of the subject.

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[*] *La Soc. de Phy. et d'Hist. Nat. de Genève. March* 1838.

This apparatus consists in a mast 114 feet high, composed of two spars of fir, accurately connected with each other; diverse precautions being taken that they should not be broken or overturned by the violence of the wind. It was placed in the most favourable situation for experiments of this kind, that is to say, in the midst of a great field, at a considerable distance from all habitations. The author adjusted, at the distance of every ten feet throughout the whole length of the mast, horizontal pieces of wood, to the extremity of which was attached a little pulley, by means of which the thermometers might be made to ascend and descend. The thermometers employed had their bulbs well covered with some good non-conducting substance, so that it might be certain their temperature did not vary during the time of their descent. Accurate notation was made, at the moment of every observation, of the meteorological condition of the atmosphere, and, in particular, of the indications of the hygrometer and of the ethrioscope.

The principal object which the author had in view in his researches may be regarded as a solution of the four following questions:—1*st*, To what extent the increase of temperature, which has been observed in proportion to elevation, during certain periods of the day, is influenced by the condition of the sky, and the agitation of the air? To determine, 2*dly*, in a precise way, at what periods of the day this increase of temperature becomes perceptible; if it remain constant, or if it have a tendency to increase during the night? 3*dly*, Does the limit of elevation, at which this increase of temperature ceases, remain constant, or does it vary according to the meteorological state of the atmosphere? And, 4*thly*, Whether the increase of temperature, as well as the limit of its elevation, remain constant, or vary according to the different seasons of the year?

The author discusses these four questions successively, and gives an account of his various observations, which were all made during the year 1837 and the two first months of the present year. The results obtained have led to the following conclusions:

1*st*, The increase of temperature as you ascend, which is most conspicuous at the setting of the sun, however variable it may be, whether as regards its intensity, or its limit of elevation, is a constant phenomenon, whatever may be the condition of the sky; with the single exception of violent winds.*

2*d*, The period of the maximum of this increase is that immediately following the setting of the sun. Starting from this time it remains stationary, or even frequently diminishes, especially when the dew is abundant.† At the time of sunrise, the increase is most frequently less than it was at sunset.

3*d*, The limit of elevation to which the increase of temperature extends, appears rarely to surpass the height of 100 feet, even when the sky is perfectly clear and serene. When it is very cloudy, and especially in winter, this limit is much lower

4*th*, The increase of temperature in ascending, varies, both according to its intensity and as to the limits of its elevation, according to the different seasons of the year. It is especially during the winter, and when the surface is covered with snow, that this phenomenon presents the most remarkable results.

The extraordinary severity of the last winter, enabled the author to make many observations, upon the remarkable difference which may exist between the temperature of different strata of the air but little separated from each other. The maximum of this difference amounted, on the 20th of January, to 14°.4 of Fahr. on a change of elevation of 50 feet. A thermometer placed at the height of two feet above the surface, indicating 3° Fahr., and another at the height of 52 feet, indicating at the same moment 17°.4. The mean difference, calculating upon twelve observations made during the period of excessive cold, between the temperature of two strata of air separated by an interval of 50 feet, was 10° Fahr. These differences were much less conspicuous during fine weather.

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* The author establishes by a great number of observations, that the phenomenon is not confined to those occasions in which the sky is clear and serene, as had hitherto been supposed. It also exists, though in a less degree, even when the sky is overcast; excepting always the periods of violent winds.

† The author has almost constantly observed that an abundant descent of dew has a tendency to raise the heat of the strata of air which are nearest to the earth, and, consequently, to re-establish, to a certain point, an equilibrium between these strata and the superior ones.

The comparison between the temperature of the air at *two* feet, and at *five* feet above the surface, perhaps presented still more remarkable results than the preceding, regard being had to their great proximity. The difference, calculating from the mean of nine observations (the surface being then covered with snow), was 4°.2 in favour of the more elevated station; this difference, on the 4th of January, increased to 7°.2 Fahr.

A great number of trees in the neighbourhood of Geneva have suffered this winter from the intensity of the frost. The gardeners have remarked, in many instances, that the lower part of the tree was frozen, whilst the upper branches remained perfectly uninjured. Localities even have been named where a great number of the trees were found frozen to the height of four or five feet, and remained green above this limit. The facts contained in this memoir of M. Marcet serve to account for these apparent anomalies.*

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* Arago, in his Instructions for the use of the Naturalists of the French Arctic Expedition, at present probably in Spitzbergen, has the following remarks:—The physical causes which concur in rendering the strata of the atmosphere colder in proportion as they are more elevated, have not hitherto been subjected to an accurate appreciation; on the other hand, we are almost compelled to suppose that there is some essential omission in the enumerations which have appeared. This being the case, it has occurred to me that the investigation of an anomaly might better lead to the detection of the deficiencies, if any exist, and furnish the means of supplying them, than any general study of the phenomenon. This was the reason why I was solicitous to call the attention of the naturalists in *La Bonite* going to the Arctic Zone, to the exception which the usual law undergoes *in serene weather*, DURING THE NIGHT; upon the THEN *increasing progression* which the atmospheric temperatures exhibit from the surface of the earth to a certain limit of elevation, which has not yet been exactly determined. At the present time this field of research appears to be enlarged. In cer-

Observations and Experiments made upon the Cause and Effects of Vinous Fermentation. By M. Cagniard Latour.*

That he may prove the high interest of the subject of which he treats, M. Cagniard Latour recalls to the recollection of the Academy, that, in the year 1800, the Physical and Mathematical Class of *the Institute* proposed the following as the subject of

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tain climates the atmospheric temperatures would appear *to increase with the height*, EVEN IN FULL DAY. I have verified this result whilst discussing, with very different views, the observations of Captains Sabine and Foster, made in the year 1823, with the intention of determining the elevation of one of the mountains of Spitzbergen, which was much isolated and very pointed.

On the 17th of July, between 4h. 30′ and 6h. p.m. the mean temperature of the air was—

At the lower station, ... + 1°.6 Centigr.
At the summit of the mountain 501 faths. high, + 1°.9;

The weather was dull, with a gentle breeze.

On the 18th of July, between 3h. 20′ and 6h. p.m.—

At the lower station, ... + 1°.9
At the summit of the mountain, . . . + 1°.2;

There was a thick mist, and a moderate breeze.

On the 20th of July, between midnight and 2 a.m.—

(Note.—It is here to be remembered that on the 20th of July the sun never sets at Spitzbergen, and that at midnight it is pretty high above the horizon. In the place where Captain Sabine observed it was about 11°.)

At the lower station, ... + 2°.4
At the top of the mountain, ... + 4°.4;

The weather was most beautiful and serene.

On the 21st of July, between 10½h. a.m., and half-past noon—

At the lower station, ... + 4°.3
At the top of the mountain. . . . . . + 3°.9;

It rained at the lower station, and the mountain was enveloped in clouds.

It will now be observed that the anomaly is not exhibited when the sky is wholly overcast. On the contrary, it attains its maximum under a serene sky. All this perfectly accords with the explanation we have given of the phenomenon in our instructions for *La Bonite*, and which are grounded upon the laws of the radiation of heat; which also leads to the supposition, that, in our own climate, when the weather is good, the temperature of the atmotheir prize: *What are the distinguishing Characters in Vegetable and Animal Matters which excite, and those which undergo, the process of Fermentation?* This question, which was again proposed in the year 1802, was withdrawn in the year 1804, as well as those proposed by the other classes, on account of an unexpected event which deprived the Institute of the funds which were intended to pay the prizes.

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* In a Report to *L'Academie des Sciences.* By MM. Thénard, Becquerel, and Turpin; July 1838.

This great question concerning the unknown action of the internal movement of fermentation, and its extraordinary results, a question which, at one and the same time, relates to physics, chemistry, and physiology, having remained so long unsolved, M. Cagniard Latour, who has been engaged with it for more

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sphere may increase and not decrease with the height, even previous *to the setting of the sun.* Certain arrangements, which for a long while I have contemplated, would enable me to subject this conjecture to a decisive proof. In the mean while, we would suggest that the Academy should solicit the members of the Northern Expedition to prosecute with continued attention the phenomenon which I have submitted to their notice. A baloon with a cord attached to it, which would convey a self-registering thermometer, and which might be made to ascend from time to time, would furnish more conclusive observations than any that could be made on a mountain, however isolated or steep. The only recommendation we would make would be, to substitute a horizontal thermometer, for those of Rutherford and Six with a moving index, whose employment can be little depended upon, on account of the violent oscillations of the baloon during the ascent and descent, and even during its somewhat protracted sojourn at its highest elevation. *

* After this paragraph was written, I found in the work of M. Pictet some observations upon atmospheric temperatures increasing with the elevation, which were made during the night, or at least when the sun was below the horizon. M. Biot has also sent me the following note, regarding the observations of General Roy and Dr Lind, upon the measurement of heights by the barometer, taken from the Phil. Trans. 1777, p. 728. After citing some observations taken at very insignificant elevations, in which, by local influences, the upper thermometer had indicated a somewhat higher temperature than the lower, the author adds, "But the most remarkable example of this kind occurred in one of Dr Lind's observations, when a thaw supervened, on the 31st Jan. 1776, to a severe frost. At Hawk Hill near Edinburgh (the lower station) at 10h. 45′ a.m., the temperature in the open air was 14° Fahr.; when at the top of Arthur Seat (the higher station) it was at 20° F. The ground, which still continued frozen, kept the air very cold below, whilst the thaw had made considerable progress on the top of the hill.—The difference of the level of the stations here specified was 684 English feet, and the excess of the temperature at the top of the column was 6°; but as the intermediate elevations were not examined, it cannot be determined whether this increase was continuous, or if a real decrease had not previously existed at the higher station.

than twenty-five years, has at his leisure resumed its consideration, and by bringing new instruments to bear upon its investigation, more especially the microscope, has obtained very remarkable results.

That he may preserve all possible order in his observations, and to simplify as much as possible the object of his interesting researches, the author has confined himself to the most important as it is the most useful of all fermentations, viz. that whose object is to convert saccharine matter into alcohol and carbonic acid gas, and which in chemistry is designated as the *Vinous fermentation.*

M. Cagniard Latour, thoroughly convinced that henceforward all chemical analyses must be preceded and illustrated by microscopic analyses, that so the nature of the bodies under investigation may be recognised, whether inorganic, or organic,* or finally organized organic, † has, as remarked, used the microscope, without whose use we can no longer speak of any body with confidence.

The microscopic analysis of that paste which is named *beer-ferment*, which in chemistry is regarded as a simple substance, and which separates from wort of beer during the fermentation as *yeast*, under the form of scum and sediment, has convinced M. Cagniard Latour that this paste, though apparently simple, is, on the contrary, when observed through the microscope, a mass entirely composed of a multitude of globular and slightly oval particles, which are vesicular, transparent, and full of smaller globules, the largest reaching to the size of about the hundredth part of a millimetre, or the three-millionth part of an English inch, without motion, and consequently, vegetable in their nature, according to our most approved definitions.

After having recognised that the vesicular globules of the yeast of beer were organized, the experimentalist was anxious to ascertain by the microscope how these little vegetables exist, and are formed in beer, and how, finally, they are multiplied so as to produce a very considerable augmentation of new ferment.

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* Organic matter, considered as an element of organization, and only impregnated with those elements of life which organized bodies possess.

† The organic matter employed temporarily in the structure of a vegetable or animal, or the organization destroyed, and reduced into a gelatinous substance.

M. Cagniard Latour inquired if the globular vegetables of the yeast were limited, as it regards their organization, to a maternal vesicle capable of reproducing and multiplying itself by small seeds, produced by extension, either from the external surface, or, what is more probable, from the internal; or whether, placed in a somewhat more elevated order of organization, they do not represent in the mass of the ferment, simple reproducing bodies, of that kind which germinate and advance to minute vessels of a somewhat more complicated nature.

The author made many attempts to arrive at a more certain knowledge of the organization and physiology of these microscopic vegetables. The first, not very adroitly executed, or possibly conducted on too small a scale, did not produce any vegetation,—the yeast decayed, and became decomposed. But it was very different with another attempt, though long and troublesome, which was made during the night in the English brewery of M. Leperdriel, in a vat containing about 260 gallons of the wort of porter.

This wort, when first examined in the microscope, only exhibited a great number of very slender particles without any determinate form; but after the yeast had been introduced for about half an hour, about ten o'clock at night, between 6 and 7 pounds of ferment having been added, the wort, when again examined, exhibited solely the vesicular globules of the ferment which had been employed, the number of which in the field of the microscope might amount to about eighteen. After this, specimens were taken from the vat every hour until six o'clock in the morning, the time required for the fabrication of the porter; and these presented successively the following transformations:—

In the first, all, or nearly all, the simple globules of the yeast which had been introduced into the wort, were observed to be supplied with one or two small buds, more transparent than the maternal globule, of which, however, they seemed extensions. Some of them had not as yet produced their bud, whilst others, more advanced than those first mentioned, were found composed of two equal globular joints, this new bud having attained the diameter of the mother or producing globule.

In the second specimen, the whole of the globules were now composed of two joints, and on some of these joints were to be seen sometimes one and sometimes two new buds, which were opposite, and placed in a contrary direction.

The inspection of six other samples taken from the vat from hour to hour, demonstrated to M. Cagniard Latour that the vegetation had steadily advanced; for in the liquid of the eighth portion a great number of objects might be discovered, formed of three, four, and even five globular joints, which had successively produced one another, and which were arranged in a series, like a string of beads. Among them many were perceived which had not made the same progress. Some of these were still nothing more than a simple globule, others exhibited upon this globule one or two nascent buds, or two or three globúles supplied, for the most part, with terminal buds. Besides, as the product of these vegetable developments, the experimentalist conceived that the number of globules was greater than in the first sample after the yeast had been put in. But on this point no doubt could remain, for the author adds, that some days later, when the whole of the yeast produced in the vat was collected, the quantity amounted to 47 pounds, nearly seven times the quantity of the ferment which had been put into the must. At this time likewise scarcely any other than simple or isolated globules were discoverable; indicating the great facility with which these minute moniliform vegetables disarticulate themselves, when the conditions necessary to their existence are no longer present.

In this set of microscopic observations, whose principal results have now been alluded to, M. Cagniard Latour states that he has remarked a difference between the appearance of the simple globules of the yeast and that of those which were developed in succession during the act of fermentation; these latter, as being younger than the former, appearing more opaque and cloudy. He also noticed, but only on two occasions, that the globules emitted, by a kind of explosion, a remarkably fine powder.

M. Cagniard Latour has remarked that the globules of yeast had a constant tendency to rise to the surface of the wort so long as the fermentation continued, and he is inclined to believe that this tendency is owing to the disengagement of the gas of the globules which are suspended in the wort. He also thinks that he has perceived that these globules, whilst agitated in the wort, diminish in size by contracting, and that, as the result of this contraction, they emit, in the liquid space, small seeds (*seminules*) or reproductive bodies, which, after having vegetated and attained the diameter of the original globule, had the faculty of developing themselves in the way of successive buds, and of so producing, as we have already said, minute moniliform vegetables. As we have seen, the author admits these two distinct methods in the reproduction and multiplication of the minute vegetable in beer-ferment,—both that by small seeds, and that by germination or budding; an observation which appears the more interesting, as it is in perfect keeping with the twofold mode of reproduction of all the simple microscopic vegetables situated at the limit of the scale of vegetation.

If M. Cagniard Latour had not thought himself obliged to answer the questions proposed about the vegetable or animal nature of the globules of the yeast, we should have ventured to have blamed him for spending too much time in discussions which appear idle; for the discussion is concerning organized productions, which, being neither common esculents, nor mammiferous animals, can be distinguished only by what they themselves really are, isolated in nature, and without regarding our conventional characters of vegetables and animals. It would have been more suitable to have said, Yeast is not a simple substance or chemical product, as has been thought. What appears a dry and soft paste is an agglomeration of vesicular globules, without locomotive power, organized, because susceptible of absorption, assimilation, and increase by the addition of joints, and finally, of reproducing and multiplying themselves; in short, according to the prevailing opinions,—a vegetable.

After having demonstrated that ferment or yeast is an accumulation of minute vegetables, or at least of bodies capable of producing them, the author proceeds to some observations which are purely chemical. He remarks, 1*st*, That yeast acting upon sugar loses its nitrogen, as is generally known; 2*d*, That all vegetables in a rudimentary state yield ammonia directly upon distillation. He then insists upon the production or augmentation of the yeast, which, for each vat, yields about seven times the quantity put into the wort. This augmentation, which might be supposed to proceed from a precipitation of the vegetable albumen which is found in the wort, M. Cagniard Latour, according to his observations, simply and positively accounts for by the multiplication of globules, as above explained, and the number of which agrees well with the augmentation of the weight.

As a proof of the vegetable organization of the globules of yeast, the author recalls to our recollection, that this yeast quickly and properly dried may, like a great many seeds, be preserved for a very long time, and is after this susceptible, when placed in favouring circumstances, as in sugar and water, of germination, vegetation, and the production of vinous fermentation. This happens, even when it has been exposed to a temperature so low as 60° cent. or below zero, Fahr. Finally, M. Cagniard Latour closes his memoir with this observation: "All those who are concerned with fermentation on a great scale, such as brewers, distillers, &c. know that in spite of all the care they can bestow, the result is always very variable. This very irregularity favours the hypothesis, that vinous fermentation is excited by a substance, which is endowed with vitality, for who does not know in how many different ways such substances may be affected."

The discovery of this author merits the most detailed examination, and with every possible care. It requires many experiments and microscopic observations, long-continued minute and repeated observations, which can neither be well explained nor comprehended, except by the help of numerous figures, which are herewith submitted to the examination of the academy. We have personally, say the reporters, submitted to this examination with the greater interest, because from the commencement we have recognised the truth of the facts announced by the author, and the vast importance of their application to physics, chemistry, physiology, and domestic economy.

Observations taken at St Bernard's Crescent, Edinburgh, on the 21st and 22d June 1838. *Height above the Sea* 110 *English feet.* By a Committee of the Physico-Mathematical Society.

Hour.		Barometer reduced to 32° Fahr.	Thermometers.			Wind.		Rain in Inches:	General Remarks.
			Attached.	External.	Moistened.	Direction.	Force.		
June 21,	6 A. M.	29,164	59°.5	50°.7	47°.9	W. by N.	Light	...	Sky clear: a few light clouds.
	7 ...	29,163	62.4	52.2	48.3	do.	do.	...	Do. do.
	8 ...	29,186	64.8	53.2	49.0	W.NW.	do.	...	Fleecy cumuli, increasing in number and size.
	9 ..	29,172	67.0	56.0	50.0	do.	Stronger	...	Do. still increasing.
	10 ...	29,203	67.5	58.5	51.5	do.	Strong	..	Do. becoming more defined in their edges.
	11 ...	29,210	68.7	59.5	49.5	NW. by N.	do.	...	Clouds fewer, and more fleecy.
	12 ...	29,225	69.8	60.0	51.5	W.NW.	do.	...	Clouds numerous and fleecy; little blue sky.
	1 P. M.	29,217	68.7	61.0	53.2	do.	do.	...	Clouds chiefly to the north.
	2 ...	29,216	70.5	61.0	51.8	do.	do.	...	Clouds smaller, and more numerous.
	3 ...	29,224	71.5	61.0	52.5	do.	Abating	...	Clouds chiefly in north and west.
	4 ...	29,220	71.0	61.0	52.5	do.	Pretty strong	...	Few clouds. [ing with different velocities; comoid cirri.
	5 ...	29,217	69.5	60.2	51.5	W.	Abating	...	Dense clouds, with rain, NW & N.NE.; two strata of air mov-
	6 ...	29,216	69.3	58.8	51.7	W.SW.	do.	...	Dense arches of clouds from W. to N.; comoid cirri N. by E. to
	7 ...	29,225	67.8	56.0	51.7	do.	Light	...	Light floating cumuli all over the sky. [S. by W.
	8 ...	29,226	68.0	56.0	49.0	do.	Stronger	...	Fine evening; cirro-cumuli and cumuli over the whole sky.
	9 ...	29,234	68.0	52.0	48.5	do.	Very light	...	Cumuli small and few bank of clouds along the horizon.
	10 ...	29,267	66.5	52.0	49.0	W.	Stronger	...	Clouds more numerous, but small; fine evening.
	11 ...	29,266	66.0	52.0	49.0	do.	do.	...	Sky nearly obscured by thin fleecy clouds.
	12 ...	29,276	65.2	52.0	49.5	do.	Moderate	...	Sky obscured by dense clouds; a few drops of rain.
June 22,	1 A. M.	29,290	65.2	52.0	49.0	do.	do.	...	Sky still dull; some small drops of rain. [of rain.
	2 ...	29,292	65.0	53.0	49.5	do.	Stronger	...	Clouds denser, obscuring the sky, and driving very fast; drops
	3 ...	29,308	65.0	53.2	49.2	do.	Brisk	...	Few cumuli; some cirri in zenith, apparently stationary.
	4 ...	29,303	63.5	52.5	49.0	do.	do.	...	Few clouds; some mares'-tails.
	5 ...	29,324	63.7	53.5	49.5	do.	Moderate	...	Clouds not numerous; in three strata.
	6 ...	29,321	64.5	54.0	50.0	do.	do.	...	Do. do.
Means,...		29,238	66.6	55.6	40.1				

A brief View of the Botany of Ireland. By J. T. MACKAY, M.R.I.A., A.L.S.*

ALTHOUGH the Flora of Ireland is not so numerous as that of Great Britain, it possesses a good many plants not found in England or Scotland, some of which may be noticed, together with others of rather rare occurrence to be found in different parts of the country.

Killarney is celebrated for its large specimens of Arbutus (*A. unedo*), and the Kerry and Cork mountains furnish several species of *Saxifrage*, of the *Robertsonia* or *London Pride* division, not found elsewhere in Britain, as may be seen by referring to the Flora Hibernica. The rare and beautiful fern *Trichomanes brevisetum*, now so much sought after, is found in greater abundance near Killarney than any other place in Ireland.† Brandon, in the county of Kerry, is one of the richest mountains in Ireland for alpine plants, near to which, on Connor Hill, the rare little procumbent plant *Sibthorpia Europæa* is to be seen in abundance. The *Pinguicula grandiflora*, found abundantly near Cork and other parts of the country, is particularly deserving of notice, as it is now much sought after by cultivators.

The wild district of Connemara in the county of Galway furnishes a considerable number of rare and interesting plants, the most remarkable of which are the following :—*Erica Mediterranea*, found on Urrisbeg, near Roundstone, which species has, since its first discovery there, also been found in Erris; *Erica Mackaiana*, *Menziesia polifolia* or Irish heath, which, as well as the beautiful variety with white flowers, are now general favourites in garden collections. The curious *Eriocaulon septangulare*, which also grows in the island of Skye, in Scotland, is here to be seen in almost every lake. The London Pride, *Saxifraga umbrosa*, is found on several of the mountains in the greatest abundance; on Muilrea mountain in the county of Mayo, on Croagh Patrick, and in Erris. *Saxifraga oppositifolia*, which grows plentifully in the Donegal and Sligo mountains, is also to be met with on the range of mountains which separates Connemara from Joyce country. The isles of Arran afford the beautiful and delicate *Adiantum capillus-Veneris*, or true maiden-hair fern, in the greatest profusion, in the crevices of the limestone rocks, of which the islands are composed. It is also found more sparingly near Roundstone and on the high mountain range between Tralee and Dingle, in the county of Kerry.

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* Drawn up for Messrs Curry & Co.'s "New Guide to Travellers in Ireland," not yet published.—EDIT.

† It has recently been found by Robert Ball, Esq. in the county of Waterford.

In a recent botanical tour through Connemara and other parts of the county of Galway, the following plants were added to the Flora of that country :—*Carex filiformis* and *Carex limosa* in boggy ground near Woodstock, four miles from Galway, on the road to Outerard, and on a small limestone hill opposite to it *Orobanche rubra*, hitherto only found on trap-rocks, near Belfast and Magilligan. The genus *Orobanche*, of which we have three species indigenous to Ireland, is generally supposed to be parasitical. One species, *Orobanche major*, grows on the roots of the common broom; hence the English name broom-rape. Another species, *Orobanche minor*, is in this country invariably found near the roots of ivy, and does not appear to differ from the species known by that name in England, which is there always found among clover. *Orobanche rubra*, however, does not appear to derive its nourishment from any other plant, but is generally found growing in the crevices of rocks.

By the side of the Outerard road, near Ross, and in Ross woods, *Pimpinella magna* was found in great abundance. A new habitat for the *Erica mediterranea* was observed by Simon Foot, Esq., Joseph Hooker, Esq., and others, on the side of Mulrea mountain near the Killeries; and on the cliffs near the summit *Oxyria reniformis* was found for the first time.

Erica M[illegible] also seen in full flower about half-way between [illegible] Roundstone, where it was originally dis-[illegible] to be a great acquisition to our garden [illegible]

[illegible] was foun[illegible]ndantly in corn-fields, and by [illegible] two mil[illegible] west of Outerard. It had previ-[illegible] observ[illegible] in the county of Donegal.

On the Burren mountains, county of Clare, the mountain of Avens, *Dryas octopetala*, which is also found in Antrim, is most abundant; and the *Potentilla fruticosa*, which is found plentifully at Rock Forest, near Gort, is also worthy of notice. Ben Bulben and the other adjoining limestone mountains in the county of Sligo are interesting to the botanist, in producing the rare *Arenaria ciliata*, together with a good many other alpine plants, some of which may be mentioned, viz.—*Silene acaulis*, *Alchemilla alpina*, *Thalictrum alpinum*, *Oxyria reniformis*, *Rhodiola rosea;* and since the publication of Flora Hibernica, *Saxifraga nivalis*, an inhabitant of the highest cliffs of Ben Lomond, Ben Lawers, and other mountains in the Highlands of Scotland, has been added to our Flora, by John Wynne, Esq. of Hazlewood.

The Donegal mountains, as far as they have been explored, do not appear to have any plants peculiar to them; but the adjoining county of Antrim contains some of the rarer productions of our island, of which *Orobanche rubra*, found on the traprocks of Magilligan and on the Cave Hill near Belfast, may be noticed, and *Arenaria verna* in the former station. On a mountain near Garvagh in the same county, Mr Moore, the able botanist attached to the Ordnance Survey, has found three species of *Pyrola*, viz:—*Pyrola media*, *minor*, and *secunda*, the only habitat in Ireland for the last-named species. Mr Moore has also found in Antrim, *Carex Buxbaumii* and *Calamagrostis lapponica*, new to the British and Irish Floras.

In the neighbourhood of Dublin, from its vicinity to the sea and mountains, a large proportion of the plants of Ireland is to be found; and the botanist will be well rewarded by visiting Howth, Portmarnock Sands, Killiney hill, and the adjoining county of Wicklow; but as the habitats of all the rarer plants are given in our Flora, it is unnecessary to enumerate them in this short sketch.

Doctor Taylor, the celebrated Cryptogamic botanist, has well described the Mosses, Hepaticæ, and Lichens of Ireland in the second part of the Flora Hibernica, from which it will be seen that our island is rich in those minute vegetables. In the last-mentioned family, the *lichenes*—he has described many species quite new, chiefly found by him near Dunkerrin, in the county of Kerry, where he now resides.

The shores of Ireland are also rich in marine plants, which are ably described by Mr Harvey in ths above-mentioned work. The late Miss Hutchins of Ballylickey has enabled us to record the many rare and interesting species found by her at Bantry Bay, as has Mr Harvey those of the coast of Clare and other places; and Miss Ball has very successfully examined the Waterford coast near Youghal. To Mr Templeton, the late eminent botanist; Doctor Drummond of Belfast; and Mr Moore, we are indebted for a knowledge of many rare species of Algæ, found by them on the Antrim coast. In conclusion, we may add that it cannot now be said that the botany of Ireland is little known.

Summary of some new Inquiries concerning the Disengagement of Caloric by Friction. By M. BECQUEREL, President of the Academy of Sciences for the year 1838.*

ALL bodies are regarded as formed by the union of an infinite number of molecules or atoms surrounded by heat, which *opposes their immediate contact;* all theoretic views concerning the nature of heat being at the same time avoided. When its quantity is increased or diminished, the distance between the atoms becomes alternatively greater or smaller, and the size of the body undergoes correspondent variations.

It is also admitted that these same atoms are subjected to an *attractive power* which tends to their mutual approximation, and which, consequently, is opposed to the repulsive action of heat. With these two a third power is finally associated in the constitution of bodies, viz. the *attraction of the atoms for the heat,* which surrounds the neighbouring atoms. So long as the power of aggregation prevails over the other two, the body continues solid; if the heat is increased, a time comes when the atoms acquire a certain degree of mobility, and the body becomes liquid. And, finally, if the quantity of heat becomes so considerable that it quite overcomes the power of aggregation, the body assumes the gaseous form. The atoms of bodies being thus maintained at greater or less distances, in virtue of the reciprocal actions of heat and the power of aggregation, they must be separated from each other by interstitial spaces in which all the phenomena of light, heat, mutual affinities, and atomic attraction play their parts. It is then in these spaces that the imponderable agents unceasingly contend with the material principles of bodies.

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* Read to the Academie des Sciences, Aug. 13. 1838.

Caloric must here exert a very powerful agency; for, according to its intensity and mode of action, it produces both light and electricity, and induces the play of the chemical affinities. Hence it follows that we cannot study too minutely the properties of this agent in relation to the particles of bodies, if we wish to ascertain its immediate influence in all that concerns natural phenomena of the very highest order. These considerations suggested the idea of a series of experimental researches which have led to some new results, of which we shall now endeavour to supply a sketch, entering as slightly as possible into those technical details which it would be difficult to follow in a hasty perusal.

We shall first direct our attention to a body in equilibrium, as it regards temperature, with the surrounding medium. If, in any way, we so disturb this body, that its atoms lose their natural equilibrium, it is evident that all the imponderable agents which are placed in the interstitial spaces will be put in motion. A crowd of phenomena is the immediate consequence, which the philosopher must endeavour to analyze with every help which science puts at his disposal. We shall first direct our attention to the effects produced by heat when friction is the disturbing cause employed.

It is well known that when two bodies are rubbed against each other, heat and electricity are disengaged. Are these effects, which are concomitant, also dependent upon each other? This we shall discuss by and by, and shall dwell now solely upon the effects of the heat.

All that we know concerning the production of heat by the mutual friction of two bodies may be reduced to this: the two bodies become hot, and the quantity of heat emitted is sometimes such that it is sufficient to set combustible bodies on fire. Thus it is, that a wheel turning rapidly on the axletree takes fire; and that many savages, with an address and dexterity which we do not possess, succeed in lighting pieces of wood by rubbing them against each other with astonishing rapidity. Every thing leads to the conclusion that the effect thus produced is owing to the vibratory motion produced by the rubbing, upon the atoms, and the following facts go to prove this supposition.

When an alloy of one part of iron and two parts of antimony is subjected to the action of a file, bright sparks are immediately produced, which proves that the temperature is raised above incandescence. The percussion of flint and steel produces a similar effect. M. de Rumford in boring a cannon placed vertically, found so much heat produced, that he thereby boiled water, in a small cavity which was favourably situated. This, then, is nearly all that we know concerning the disengagement of caloric through the agency of friction; and thus it would appear that we are completely ignorant of the part which each of the bodies plays in the production of the phenomenon, both as it respects its inherent nature, and the state of its surface.

That we may determine how far each body is concerned, we must endeavour to separate all the causes which obscure the effect we are investigating, although, unfortunately, it is scarcely possible to carry this purpose into execution. In fact, when we rub two bodies against each other, more or less rapidly, their contact being continuous, there is an evident transmission of heat from the one body to the other. The quantity which is transmitted from each of them, depends upon the conducting power of the body, also upon its capacity for caloric, and upon the state of its surface. Moreover, the caloric disengaged from one of the bodies cannot be immediately ascertained before its transmission into the other, with the common thermometers, because their indications are not instantaneous. But, notwithstanding all this, means may be devised by which we may succeed in operating in circumstances which will overcome many of the difficulties at which we have hinted, and the following facts are some of the results of these arrangements.

The apparatus with which these effects were observed, consists of a thermo-electrical pile, having an excellent multiplicator. Such is its sensitiveness, that the differences of about a hundredth part of a degree, of the Centigrade thermometer, between the two faces of the pile, causes the needle to traverse so much that the angle of difference is quite appreciable.

To reduce the question to the most simple expression possible, two bodies of the same nature were selected, which were bad conductors of caloric, which were equal in all their dimensions, and which presented no other difference than that which arose from the different conditions of their surfaces. These bodies were conveniently fixed on glass stalks. The rubbed surfaces were each placed in contact with one of the faces of the pile. When the two surfaces have the same temperature, the needle remains in repose, owing to the circumstance that the two thermo-electrical currents are equal, and flowing in opposite directions, they destroy each other. But when the temperature is not the same, the needle speedily traverses, and the angle of movement enables us to measure the difference of the temperature. The friction, by means of suitable apparatus, is produced with a determined velocity and precision, so that its intensity is always known. The two bodies can easily be separated instantaneously from each other, and can be immediately subjected to the required examination. These are the methods by which the experiments were made, and we now proceed to the results.

We begin by inquiring into the effect produced upon the needle by the contact of one of the rubbed surfaces with one of the faces of the pile; in short, with the effect produced by the heating of this face.

Experiment proves that, whatsoever is the nature of the rubbed disk, whether it be a conductor or a non-conductor of heat, the time which the needle requires to attain its maximum of traversing, provided the traversing does not exceed 60°, is always 10″. For a traversing to the extent of between 60° and 75°, it takes 9½″, and 9″, for deviations which reach from 75° to 90°.

The needle, then, in this respect, corresponds to a pendulum which oscillates, under the action of a weight, within narrow limits, since the deviations are isochronous; at the same time, with this difference, that in the pendulum, when the amplitude of the oscillation increases beyond a certain limit, the time of oscillation also increases, whilst the contrary occurs in the experiments we are describing, that is to say, that the time diminishes in proportion as the increase augments from beyond 60° to 92°. This result is connected with the propagation of heat and electricity in bodies.

Let us now take two bodies of the same nature, quite equal in size, and arranged as already described. Let us, for example, take two disks of cork, the one of which shall have its surface smooth, and the other made thoroughly rough. If one of these be rubbed against the other in a regular and steady manner, and they are then presented simultaneously to the two faces of the thermo-electrical pile, the needle immediately traverses, and its direction indicates that the roughened disk has acquired more heat than the other, and this in a ratio which varies with the rapidity of the friction. Precisely the same result follows when a piece of polished glass is rubbed upon a portion of rough glass. In the circumstances in which we have operated the former has acquired only half the heat of the latter. It follows then, that the absorbing power of bodies exerts an influence upon the disengagement of caloric during friction. This law, however, does not appear to be general, for white satin acquires more caloric than black satin, which has a greater absorbing power.

If bodies of different natures be submitted to experiment, the following results are obtained. Experiment 1*st*, with polished glass and cork;—here the former substance acquires more heat than the latter, in the ratio of 34 to 5. 2*d*, with rough glass and cork;—here the ratio is as 40 to 7. 3*d*, with silver and cork;—the ratio in this case is as 50 to 12. 4*th*, with caoutchouc and cork;—the ratio here is as 29 to 11; and so with many more. Regarding these, we observe that the numerous results which we have obtained from the friction of different bodies, do not supply us with any simple laws, on account of the many different causes which all bear on the general result. It appears only, that *the nature of a body*, an abstraction made, distinct from its conducting power, exercises an influence which the state of the surface does not always destroy. We have found it impossible hitherto to discover the cause of this influence, which depends on the nature of the bodies, and probably upon the arrangement of their atoms. But it is no small matter that we have exhibited it by experiments, for we are thus supplied with another element, which henceforward may be taken into consideration in the theory of heat.

If we now inquire concerning the relations which exist between the production of heat and the production of electricity, in the mutual friction of two bodies, the following are the consequences which result from the experiments which have been recently made. The displacement of parts of the rubbed surfaces always occasions a disengagement of heat and a disengagement of electricity,—two effects which exert a mutual dependence. This dependence, however, is so much obscured, that it is still impossible to affirm if the one precedes the other, or *vice versa.* We can only make conjectures on the point,—conjectures which go to shew that the heat is derived from the electricity, when the bodies are of the same nature,—are bad conductors of caloric,—and only differ as to the condition of their surfaces. The surface which is most heated becomes negatively electrified; and that which is least heated positively. When the bodies are different, the effects become more complex, and can be interpreted only when the results are immediately under observation.

Some facts recently brought under review permit us to group together the relations discovered between heat and electricity, and of which phosphorescence supplies an example. It is known that this phenomenon shews itself wherever particles of bodies which are bad conductors of electricity are disturbed by percussion, friction, heat, light, a shock of electricity, or when they are decomposed by chemical action. These causes are precisely those which likewise disengage electricity; and the phenomena being atomic, must produce an infinite number of minute sparks which together produce a faint light similar to phosphorescence. Hence we may suppose that phosphorescence has an electrical origin.

In glow-worms (*Lampyris*) and the infusoria, we are ignorant whence the phosphorescence proceeds, and if it be owing to electricity. The important experiments, however, of M. Ehrenberg are about to instruct us. This able physiologist has lately been studying with peculiar care the light which is emitted in darkness by the infusoria and the annelides which make the ocean luminous in certain countries, especially when its surface is agitated by a gentle breeze. Having placed on the object-glass of his microscope, water containing these animalcula, he was exceedingly astonished to perceive, that the diffuse glimmer which surrounded them was nothing else than a collection of a vast number of small sparks which came from every part of their bodies, and particularly from the bodies of the annelides. These sparks, which succeeded each other with great rapidity, had such a resemblance with those we observe in common electrical discharges, that M. Ehrenberg does not hesitate to conclude that they are identical. He has also satisfied himself that the light emitted is not owing to a particular secretion, but solely to a voluntary act of the animalcule, and that it shews itself as often as it is irritated by mechanical or chemical means, that is to say, by agitating the water, or throwing either alcohol or acid into it. This is an additional analogy with the torpedo, which only gives a discharge when it is irritated. In the animalcula, as in the torpedo, it is also observed that the discharge recommences after a certain time of repose. From this similarity of effects, in the same circumstances, may we not infer an identity as to the causes? Now, in the torpedo, it is already known, and no one longer doubts it is electricity; and, hence, we must admit that electricity is also the cause which produces the phosphorescence of the infusoria and the annelides. It is sufficiently remarkable that the luminous or other phenomena which depend upon electricity are so much the stronger in proportion as the animals are smaller; and it would appear that this profusion of the electrical fluid, which is emitted only by beings of an inferior order, is destined to discharge other functions in beings of a higher order.

Is it not, after this, allowable to imagine, as M. Berzelius and other philosophers have advanced, that the light which is disengaged by combustion, and which occasions so great a disengagement of electricity, is, also, nothing more than the result of the discharge of an infinite number of small sparks produced in the combination of combustible with other burning bodies?

We perceive, therefore, that the relations which associate together light, heat, and electricity, acquire from day to day additional extension, and demonstrate that these three agents which rule in the atomic constitution of bodies, are derived, according to all appearances, from a single principle of an ethereal nature, spread throughout space and through all bodies.

On Professor Ehrenberg and Hausmann's Discoveries regarding two varieties of Siliceous Earth found near Oberohe in the Hanoverian Province of Lüneberg.

On the 8th January 1838, Professor Hausmann communicated to the Royal Society of Sciences of Göttingen, a preliminary notice, on a discovery connected with our own country, which is undoubtedly among the most remarkable facts lately added to the science of geognosy.

In the month of November this year, Colonel von Hammerstein, President of the Provincial Agricultural Society at Uelzen, in the territory of Lüneberg, the able author of several prize essays, and the zealous promoter of the agriculture of his native country, had the goodness to send to Professor Hausmann two specimens of varieties of earth, which were dug out near Oberohe during an excavation made by the above-mentioned society in the district of Ebstorf. The extreme lightness of these varieties of earth rendered it improbable that they were of an argillaceous nature; but their state of aggregation did not permit us to conclude that they consisted of pure silica, although, notwithstanding this, they really have such a composition, according to the chemical examination kindly instituted by Dr Wigners in the academical laboratory. The specimen No. 1. according to this investigation, is chemically *pure silica*. It has, at the same time, a fine, extremely loose, earthy, flaky consistence, and a chalk-white colour. It has a soft and meagre feeling, somewhat like starch, and does not grate between the teeth. On water it swims for a moment, then sinks down, and gradually swells up. Mixed with a little water, it acquires a pasty consistence, without being adhesive. The specimen No. 2 is also silica, but contains likewise a very insignificant quantity of a matter destructible in fire. Its fracture is fine-earthy; the colour brownish-grey, slightly inclining to green, becoming darker by the addition of moisture. It is friable, meagre, but soft to the touch, and adheres to the tongue. It swims on water for some minutes, but it afterwards sinks, absorbing water with a noise, giving out many air-bubbles, and then expands gradually by irregular splitting of the laminæ, without being altogether separated. When exposed to heat, it rapidly assumes a white colour. Here and there it is traversed by veins of a pure, chalk-white, fine-earthy silica, filled with smaller or larger cavities.

According to the information communicated by Colonel von Hammerstein to Professor Hausmann, this silica has been found in astonishing quantity in six different places of the above-mentioned district, on the edge and first acclivity of the great plateau of the *Lüneburger Haide*, covered to the depth of only one foot and a half by the soil. The pure white silica forms the upper bed, and has a thickness of 10 feet to 18 feet. The coloured portion is beneath, and has been already penetrated to a depth of 10 feet, without the lower boundary having been reached.

The peculiar state of aggregation of this silica led Professor Hausmann to conjecture that it might be analogous to the *Kieselguhr* found in the turf at Franzensbad in Bohemia, and that, like that substance, it might be composed of the siliceous shields of infusory animals. A preliminary microscopic examination seemed to confirm this notion. In order to attain certainty on this subject, Professor Hausmann sent specimens to the distinguished investigator of the infusory world, Professor Ehrenberg of Berlin, who, by his extraordinary discoveries regarding the occurrence of fossil infusoria, has opened an entirely new field of the most interesting investigations. He requested that naturalist to examine these specimens of earth more minutely, with a special view to these objects, and he received, through his kindness, the intelligence, that *both earths are entirely composed of beautiful and perfectly preserved infusory coverings;* that these are very various, but still belong only to known species, and to such as are found in a living state in fresh water at the present day. In the earth No. 1 they are free from foreign admixture; but in No. 2 they are mixed with organic slime, and with the pollen of pines. During even his first examination, Professor Ehrenberg succeeded in determining several species of infusoria, whose coverings form this silica, and in ascertaining that there occurs, in the lower bed, a species of infusoria found in the polishing slate of Habichtswald and Hungary; and another peculiar to the *Kieselguhr* of Bohemia; both of which seem to be entirely wanting in the upper bed: but upon these points we shall defer further remarks, in order that we may not anticipate the publication of the completed investigation of Professor Ehrenberg.

That a mass more than twenty feet in thickness should consist almost entirely of the coverings of animals which are invisible to the naked eye, and which can only be recognised with the assistance of a high magnifying power, is an extraordinary fact, and one which the mind cannot fully comprehend without some difficulty. The farther we attempt to pursue the subject the more we are astonished. That which occurs in an invisible condition in the fluid element, and which cannot be recognised by the human senses without the assistance of art, becomes, by immense accumulation and solidification, one of the circle of phenomena which are witnessed by us in the ordinary way; a compact mass is formed, which can be weighed, felt, and seen; and this mass is presented to us in such quantity, that, when regarded only in *one* direction, it surpasses by three times the height of the human figure. Who could venture to calculate the number of infusory animals which would be required to produce even one cubic inch of this mass? And who could venture to determine the number of centuries during which the accumulation of a bed of twenty feet in thickness was taking place? And yet this mass is only the product of yesterday compared with the other more compact siliceous masses for which the infusoria of a destroyed creation afforded materials. But what would have become of that loose, light silica,—which, by its great porosity and its power of absorbing water in quantity, in some measure indicates its origin,—if, instead of being covered by soil one foot and a half in thickness, it had been covered by a great mass of earth or rock; or if another power, such as the action of fire, had caused its solidification? In that case, we should have had no bed twenty feet in thickness, but should perhaps have found a compact stony mass, capable of scratching glass, affording sparks with steel, and polishable,—a substance, which, were it not for the abundant evidence furnished by the discoveries of Ehrenberg, it would be still more difficult to suppose had resulted from the coverings of invisible animals. Such a consolidation and hardening of this loose silica, might perhaps be partly accomplished in another way, by making the experiment of employing it for the manufacture of glass, or as one of the ingredients in porcelain; by which means a discovery so very remarkable in a natural-historical point of view might at the same time become of practical importance. *Glass formed from the coverings of infusory animals!* Who would a few years ago have believed in the possibility of this substance, by whose assistance invisible life in water is revealed to us, being prepared from a material derived from the same world of extremely minute animated beings; or that we should be enabled, by means of a substance furnished by an invisible creature, to investigate the smallest and most obscure, as well as the largest and most remote bodies in creation?—(Communicated to us by Professor Hausmann from the "*Göttingische gelehrte anzeigen,*" 25th January 1838.)

On the Last Changes in the relative Levels of the Land and Sea in the British Islands. By JAMES SMITH, Esq. of Jordanhill, F.R.S.L. & E., F.G.S. & M.W.S.[*] 94

THE occurrence of recent marine remains at higher levels than those at which they could have been deposited by our present seas, early attracted the notice of the Wernerian Society; and its memoirs contain a valuable collection of facts illustrative of this subject. The communications of Messrs Stevenson,[†] Bald,[‡] Home Drummond,[||] Blackadder,[§] and others,[¶] furnish numerous observations respecting indications of changes in level on the eastern coasts of Scotland, whilst those of Captain Laskey[**] and Mr Adamson[††] record similar phenomena in the basin of the Clyde and Lochlomond.

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[*] By permission, from vol. viii. of "Memoirs of the Wernerian Natural History Society," about to appear.

[†] Wern. Mem. iii. 327.

[‡] Ib. i. 483; and iii. 125.

[||] Ib. v. 440.

[§] Ib. v. 424, 572.

[¶] Ib. ii. 342, 348; v. 572, 575.

[**] Ib. iv. 568.

[††] Ib. iv. 334.

My attention was first called to the subject by the discovery of marine shells, agreeing in general with those of the adjoining seas, embedded in blue clay, at Ardincaple, the seat of Lord John Campbell, in Dumbartonshire. At that time it was usual to ascribe all such appearances to diluvial action; and although the shells bore no marks of violent transportation, the bivalves being entire, with the epidermis uninjured, and in their natural position; yet, as the distance from the sea was small, I imagined they might have been protected from injury by having been lodged in an eddy. Two of the shells appeared to differ from any known species; one of them, a Tellina (*T. approxima*), is so common, as in many localities to become characteristic of this deposit. It resembles the *T. tenuis*, but is distinguished by a brown epidermis. The other resembled a *Natica*, but was destitute of the umbilicus. The only specimen procured of this shell I unfortunately broke, but not until a sketch of it had been taken.* Lord John Campbell was kind enough to order a new excavation to be made, in hopes of finding other specimens, but without success.

Soon after this, Mr Thomas Thomson gave an interesting description of a similar deposit at Dalmuir in Dumbartonshire, in the Records of General Science.† He collected twenty-nine species, which were submitted to the inspection of Mr Sowerby, who pronounced three of them to differ from any known recent British shells; one of them was said to be *Natica glaucinoides*, a crag fossil; another, *Fusus lamellosus*, which had only been observed about the Straits of Magellan; and a third, *Buccinum striatum*, an unknown species. This remarkable fact, coupled with my own observations, led me to imagine that the term "recent," which had usually been applied to such deposits, was perhaps not rigidly correct. In order to ascertain how far it was so, I determined to collect as many of the shells belonging to them as I could. In a fresh excavation I made at Dalmuir, I increased the number of species, from that locality alone, to upwards of seventy. The Rev. Mr Landsborough of Stevenston, in Ayrshire, was kind enough, at my request, to collect marine remains from the elevated shelly deposits in his parish; and Mr Witham sent me a collection from similar beds on the Yorkshire coasts. In order to render the comparison between the existing and more ancient races of *testacea* as exact as possible, I determined, at the suggestion of Mr Lyell, to avail myself of the facilities which the possession of a yacht afforded, to collect and form a catalogue of those now existing in the same seas. Amongst the shells dredged up, several new species have been discovered. I failed, however, in finding any of the unknown subfossil ones. As by far the greatest number of the shells, from the ancient deposits, have been found in the basin of the Clyde and north of Ireland, I have confined the catalogue of recent shells to those which are now to be found in the same seas; a comparison of the two catalogues will thus shew how far their former inhabitants coincide with the existing species.

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* Plate XLV. Fig. 18.

† Vol. i. p. 131.

In the prosecution of this inquiry, I discovered marine remains so frequently, that any attempt to describe or enumerate the localities would exceed the bounds of this paper. When once I was furnished with a clue, I found them in places where their presence had never before been suspected; sometimes in great numbers, whilst at others the very same beds were altogether destitute of them. This is peculiarly remarkable in a finely laminated clay, which I have traced to a great extent in the counties of Lanark, Renfrew, and Dumbarton. It is equivalent to the carse clay of the Forth and Tay, and must have been deposited at the bottom of a tranquil sea, at such a depth as not to have been disturbed by the agitations of the surface. The shells and other marine remains with which it abounds are almost invariably found in the lower part of this bed, a circumstance which can only be accounted for by supposing a sudden depression, which has converted a half-tide deposit into a deep-sea one. The testaceous animals have thus been entombed alive in the beds subsequently formed, and their remains are preserved with all the perfection of recent specimens. Associated with this clay, we frequently find extensive beds of pure gravel and sand also destitute of organic remains, although there can be no doubt of their marine origin. Mr Lyell has observed the same thing in similar beds in Sweden.*

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* Phil. Trans. 1836, pp. 11 and 15.

We must be cautious, therefore, in concluding that alluvial beds in which we do not find such remains, are fresh-water ones; and, of course, equally so in deciding on their marine origin, till confirmed by the presence of their appropriate remains.

These deposits are much more extensive, both as to the amount of change of level and superficial extent, than has been generally supposed. We have conclusive evidence that the whole of the British islands have, at periods which, geologically speaking, are by no means remote, been subjected to changes both of depression and elevation. The submarine forests which have been observed on so many parts of our coasts, are proofs of the former kind of changes, whilst those of elevation are evidenced by raised beaches, sea-worn cliffs and caves, stratified beds of sand, gravel, and clay, and, above all, by the marine exuviæ which they contain.

The deposits thus formed must, in Scotland at least, be intercalated between the two first groups in Mr De la Beche's classification of rocks, viz., the modern group and the erratic block group. We infer that they are posterior to the latter, from their superposition, and that they do not belong to the former, from the absence of the remains of man or of works of art. The erratic block bed, which has also been termed diluvium, has in Scotland received the provincial name of *Till*. It is very accurately described by Mr Bald,* under the name of the old alluvial cover, in his paper on the coal formation of Clackmannanshire. It generally consists of stiff unstratified clay and gravel, confusedly mixed with water-worn masses, and also with angular fragments of sandstone, shale, and coal, which have not suffered from attrition, although comparatively soft in their structure. Organic remains are excessively rare in it. Mr Bald, who remarked this, afterwards found the tusk of an elephant embedded in it in the excavation of the Union Canal; but, unwilling to draw an important inference from a solitary fact, he supposed it might have been placed in the situation in which it was found, from some accidental cause. Since that time, however, elephants' bones and tusks have been found near Kilmarnock, and at Kilmaurs, in Ayrshire. I am assured by Dr Scouler of the Royal Society of Dublin, and Dr Cowper of the University of Glasgow, who visited these localities, that, in both instances, they were embedded in the *till.* At Kilmaurs they were associated with sea-shells; and, on one occasion, I also found shells embedded in it, much broken, and deprived of their colour. Mr Trimmer, in describing the diluvial deposits in Caernarvonshire, in the proceedings of the Geological Society,* states that he found broken shells in the diluvium of the low cliff near Beaumaris. He also found broken shells *in a bed of sand*, on the summit of Moel Tryfane, 1400 feet above the level of the sea. The expression seems to imply alluvial rather than diluvial agency. Mr Trimmer, however, informs me that he ascribes their presence at so high an elevation to the latter cause, the beds having all the appearance of violent action, and the subjacent rocks worn and scratched by friction of transported pebbles. Mr Phillips also is inclined to think that in Holderness the irregularity of deposition of the shelly gravel seems to point to diluvial currents rather than to change of level. †

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* Wern. Mem. vol. i. p. 481; vol. iii. p. 126.

It is not, therefore, a necessary inference that the mere discovery of sea-shells at high levels is a proof of permanent submergence. Their fragments, like those of coal, sandstone, and shale, mark that the distance from which they have been transported is a short one. It is only when found *in situ* in regularly stratified beds that we are entitled to draw such conclusions; but their presence in diluvial beds must be held as an exception to the general rule.

Although this is not the place to offer any speculation respecting the origin of the till, I think it evident that it must have arisen from causes altogether different from those which have produced the marine alluvia. Whatever they were, they must have been violent and transitory. Of their violence we have ample proof in the size of the fragments they have transported, as well as the erosion of the rocks over which they have passed, but that they suddenly ceased must be inferred from the confused manner in which the different parts of the till are arranged. Submarine currents might indeed have moved the largest boulders, but they must have been deposited somewhat in the order of size and gravity; the sand, clay, and smaller fragments being swept forward till the diminished velocity of the current was unequal to bear them farther, and banks of gravel, sand, and clay, would be formed. No inference can therefore be drawn from it as to the former level of the land, as rushes of water capable of producing such effects must have disturbed the alluvial covering both above and below the surface of the sea.

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* Vol. i. p. 332.

† Treatise on Geology, p. 198.

All observers concur in supposing that the cause which produced the diluvial covering of the great-coal field of Scotland, must have had its origin to the westward, modified, however, by the form of the ground. Near Glasgow, it is quite evident that its action must have been from the north-west. In levelling a mass of it, the workman laid into a heap all the boulders which were too large to be lifted by the spade: this afforded an opportunity to estimate the relative proportions of the different rocks, which I found to be as follows:—

White sandstone and shale,	60	per cent.
Trap, - - -	30	...
Clay-slate and greywacke,	10	...
Granite, - - -	1	...
	101	

The sandstone was evidently derived from the subjacent coal-formation, the trap boulders from the Kilpatrick hills, which are about ten miles to the north-west, their identity being proved by the zeolitic minerals which they contained; the slate and greywacke from hills in Dumbarton and Argyleshires, about double that distance; the granite blocks must have been transported from still greater distances. Beyond the Kilpatrick Hills the trap and white sandstone boulders disappear, and are replaced by greywacke, clay-slate, and red sandstone, whilst those of granite and mica-slate become numerous. Near Helensburgh, twenty-three miles to the north-west of Glasgow, the granite strongly resembles that of Ardnamurchan. At Roseneath, I have seen rolled fragments of a compact reddish granite so much resembling that of Inverary, as to leave little doubt of its identity. In all of these cases, the bearing of the supposed original rocks is north-west, but in all of them the intervening space is intersected by deep arms of the sea and steep precipitous mountain ranges. It appears to me, therefore, that the till is as ancient as the period of their elevation, and was most probably caused by the violent geological action by which it was accompanied.

It is, at all events, in Scotland anterior to the marine alluvia which I am describing, and which have been observed reposing upon it in many places. It is proper, however, to observe, that in some instances we find stratified alluvium below the till. I have observed this near Glasgow, and on the west coast of Ireland; and Mr Bald, in describing that of the Forth, remarked, that in one case, when it was cut through to the depth of 162 feet, the lower bed appeared to have been deposited in water in the most quiescent state, as it was divided into the finest laminæ. In neither of these cases were marine remains detected, but Mr Mantell has described an ancient beach as passing under the elephant bed in Sussex, and Sir Philip Egerton found a bed of shells under the ordinary sand diluvium of Cheshire.* These facts do not invalidate the conclusion, that the changes in the level are posterior to the deposition of the till; they only prove that it has not swept away the whole of the pre-existing alluvia. I have observed the marine beds resting on Till near Glasgow, and in the excavations of the railway from Edinburgh to Newhaven. Mr Thomson has observed it in Dumbartonshire.† At Johnstone, near Paisley, in digging a well, a marine deposit, containing the bones of fishes and sea-fowl, the claws of crabs, sea-weed, and shells, was found to rest upon a bed of it, upwards of 70 feet in thickness. Mr Robberds‡ and Mr Rose ‖ have observed the same order of position in the county of Norfolk.

We can, therefore, have no hesitation in considering that in these localities changes of level have occurred posterior to the deposition of the diluvial covering, although it is not improbable that in some parts of the British islands it may have been lodged on the surface subsequent to the period when the sea had become stationary at its present level. I am inclined to think that this has been the case on the west coast of Ireland; in the counties of Clare and Kerry I observed no stratified beds above the diluvium, and, on the shores of the Shannon which divides them, no terraces except those forming at present. These facts, however, seem rather to prove different periods of diluvial agency than of elevation and depression.

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* Proc. Geol. Soc. vol. ii. p. 190.

† Records of General Science, i. 132.

‡ Phil. Mag. Oct. 1827, p. 281.

‖ Ib. Jan. 1836, p. 34.

The changes of level must have taken place anterior to the historic period, which in this country dates from the invasion of the Romans. Diodorus Siculus,[*] who wrote during the reign of Augustus, describes St Michael's Mount in Cornwall under the name Ικτις, as an island connected with the mainland by a space covered every tide, but dry at low water,—a description which would apply accurately at the present day. In Scotland, the Roman wall, which crosses the island from sea to sea, has evidently been formed at both ends with reference to the present level. The same observation applies to British tumuli and vitrified forts, which are perhaps of still greater antiquity. It is, therefore, highly probable, that no changes of level have taken place since the British islands have been tenanted by man.

We have ample proof that traces of these changes occur in every part of our coasts. In England, the observations of Messrs Phillips,[†] Rose,[‡] Robberds,[||] Sedgwick,[§] &c., on the east coast; Messrs Mantell,[¶] De la Beche,[**] Sedgwick, and Murchison,[††] on the south; and Sir Philip Egerton, Messrs Murchison, Gilbertson, &c.,[‡‡] on the west, shew, that in all parts of the English coasts they are to be met with. In Scotland, in addition to the Notices in the Wernerian Memoirs, already adverted to, the Statistical Account abounds in direct or incidental notices of similar phenomena. My own observations, and those of every well qualified observer, confirm their universality in this part of the island.

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[*] Diod. Sic. Book v. Quoted in Thomson's Outlines of Mineralogy and Geology, vol. ii. p. 45.

[†] Geol. of Yorkshire, vol. i. p. 23. [‡] Phil. Mag. Jan. 1836, p. 30.

[||] Phil. Mag. March 1827, p. 223, &c.

[§] Geol. Soc. Proceedings, vol. i. p. 409.

[¶] Geol. of Sussex, 285. Geol. Proc. ii. 203. [**] Geol. Manual, 149.

[††] Proc. Geol. Soc. Dec. 1836. [‡‡] Fourth Report Brit. Assoc. p. 654.

In Ireland, I have seen them on the east, north, and west coasts. I am informed by Mr Griffiths, that he has observed them in Cork and Waterford, and Captain Portlock has recently found them in stratified beds at an elevation of 400 feet. Proofs of such changes have also been observed in the Channel Islands and on the opposite shores of the Continent, all probably referable to the same geological epoch.

These marine beds have been discovered at every elevation, from that of the present level of the sea to a height of at least 400 feet above it; and in the solitary instance of Moel Tryfane shells have been found at the height of 1400 feet; but as the cause of their occurrence in that situation is doubtful, we may conclude that the highest elevation at which proofs of such recent changes have been hitherto discovered is limited to 400 feet.

At this height Mr Gilbertson found sea-shells in stratified beds of gravel and sand near Preston in Lancashire. Mr Murchison,* who visited this locality, observed " similar phenomena over a very considerable tract of country occupying the ancient estuary of the Ribble. Sands, marls, and gravels, occasionally forming terraces, are spread over this great area, sometimes in finely laminated beds, but for the most part loosely aggregated, and bearing a great resemblance to the arrangement of the same materials now in the act of formation on the adjoining shores. Many of the shells found in these beds far inland, and at heights extending to 300 feet above the sea, are perfectly identical with existing species." Mr Murchison justly infers, that such appearances must be ascribed to actual elevation rather than to the action of diluvial currents. Sea-shells were found by Mr John Craig, mineral surveyor, at Airdrie about ten miles to the east of Glasgow, at the height of about 350 feet; they were found between a mass of blue till and a bed of yellow stratified clay, which rested upon it. Mr Craig was inclined to suppose they belonged to the till, the shells having been filled with blue clay; but I have observed the same thing in shells which certainly belonged to the stratified deposit, and it is easily accounted for. The action of the sea upon such a bottom would naturally stir up the clay so as to fill dead shells. Those found in this locality do not bear marks of violent transportation, and the distance from the sea is so great that it is difficult to suppose that such fragile shells as the *Mytilus edulis* and *Tellina approxima* could have been borne along uninjured by diluvial action. I am, therefore, inclined to consider, that the shells found at Airdrie belong to the alluvial beds, and have been confirmed in this opinion by having had some specimens of the *Tellina approxima*, a species which has only been found in this deposit, sent to me from the same locality.

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* Address to Geological Society, Feb. 1832.

Mr Prestwich* also found, at the height of 350 feet, in beds of sand, gravel, and clay, at Gamrie, near Banff, the following recent shells: *Astarte Scotica, Tellina tenuis, Buccinum undatum, Natica glaucina, Fusus turricola, Dentalium dentalis.* They were extremely friable, but perfectly uninjured.

The promontory of Brayhead, in the county of Wicklow, is formed by a cliff of alluvial strata of coarse gravel and sand, containing sea shells; it is at least two hundred feet high, and the hill of which it is a part, and which is evidently composed of the same beds, is perhaps a hundred feet higher. Here, therefore, this deposit reaches to the height of three hundred feet. At Howth, on the north side of Dublin Bay, are similar cliffs, at the height of about a hundred feet, also containing shells and other marine exuviæ.

In the Isle of Sheppey,† recent shells have been found in a bed 140 feet above the present level. In Norfolk,‡ and in Yorkshire,‖ they have been found at the height of a hundred feet. Near Berwick, Mr Milne§ observed a tract of table-land at the height of a hundred feet above the level of the sea. It consists of vertical strata, which have all had their edges worn down to a level plain, just as would have been the case if the rocks had been exposed to the action of marine currents incessantly sweeping over their edges. When the tide is far out, exactly the same appearance is presented by the vertical rocks which form the bottom of the shore for a considerable distance out from the existing cliffs, and were there to be an elevation of the coast, another table-land would be formed exactly resembling, but a hundred feet above, the former.

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* Proceedings Geol. Soc. May 3. 1837.

† Ib. vol. i. p. 410.

‡ Phil. Mag. Jan. 1836, p. 30.

‖ Phillip's Geology, p. 198.

§ Fourth Report Brit. Assoc. p. 638.

In the basin of the Forth, beds of razor-fish (*solen*), and bones of the seal, have been found at the height of ninety feet.* At that of seventy feet, marine remains have been found on the banks of Loch Lomond,† on the Yorkshire coast,‡ in Devonshire,‖ and in the Island of Skye.§ I have found them in several localities in the basin of the Clyde, at the height of from seventy feet to the present high-water mark.

At an elevation of about forty feet there has been observed on many parts of our coasts a series of raised beaches and terraces, which, by their magnitude, indicate the prodigious length of time at which the sea level must have been stationary at this height; and if we may judge of its duration from the relative size of the ancient terraces with those now forming, it must have exceeded the recent period of which two thousand years is but a part, by an immense amount; but this is but one of the epochs in the history of this formation; between the great terrace and the sea several subordinate ones and beaches have been observed, each of them marking long continued periods of repose, whilst a sudden deepening, two or three fathoms below the low-water mark, is probably caused by another line of terraces now covered by the sea.

The great terrace, the base of which seems very generally to be between 30 and 40 feet above the sea, forms a marked feature in the scenery of the west of Scotland, in those parts where the violence of the Atlantic has not swept away the plateau of marine alluvia which, in the less exposed situations, is always interposed between it and the sea.

The northern part of the county of Ayr, which is composed of a coarse red sandstone or conglomerate, has been worn by the former action of the sea into a magnificent range of cliffs, in some places rising to the height of 300 feet; the two islands of Greater and Lesser Cumbra, lie opposite to it, and have corresponding terraces. The former of these islands is composed of the same sandstone intersected by trap veins; both the trap and sandstone have been worn away, but in different degrees, and the dykes are left standing out from the cliffs like ruined walls, affording no doubtful evidence of the length of time during which the sea formerly washed their bases.*

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* Wern. Mem. vol. v. p. 572.

† Letter from Mr Buchanan of Arden.

‡ By Mr Witham of Lartington. Phillip's Geol. of Yorkshire.

‖ Geol. Soc. Proc. Dec. 14. 1836.

§ M'Culloch's Western Islands, vol. i. p. 293.

Similar phenomena have been observed in Jura, Mull, and Isla, at elevated levels, as well as at that of our present seas, and they furnish, as Mr M'Culloch observes, " the most perfect record which geology affords of the wasting action of the sea upon the land."† After remarking that the destroying causes of such as are found on our present shores are so obvious that it would be superfluous to point them out, he offers the following speculations on the origin of those in question. " The other case, that of outstanding inland dykes, such as those of Cumbray, and the more conspicuous examples in Isla and Mull, is more difficult of explanation; it is equally evident, however, even in these two instances, that the surrounding strata must once have existed at least at the same level as the summits of the present dykes. Nor can any obvious causes now be traced by the operation of which so great a removal of land has been effected; there are no rivers in any of the instances enumerated, to which it could be attributed, nor, indeed, could any action of a river be imagined capable of producing those effects on surfaces so irregular." He supposes they may have resulted from the tedious operation of the atmosphere, but the actual change of level affords an easy solution of the difficulty, and in each of the cases cited, we have the additional evidence of such an origin from marine remains, embedded in the alluvial strata which accompany them.

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* The time is not yet gone by with geology, as it has with astronomy, when the conclusions drawn from its phenomena are supposed to be inconsistent with the word of God. I rejoice, however, to feel assured that, in yielding to evidences which it is impossible for me to resist, I am neither denying its truth, nor wresting it to my own purposes. That interpretation, which admits, to the fullest extent, the remoteness of the " beginning," was not invented to meet a geological difficulty, but has been held by learned and pious men of all ages. To those who, unacquainted with the science, think the conclusions drawn from its investigation too uncertain, and too contrary to each other, to be worth attending to, I would say, that such discrepancies of opinion are every day disappearing as the science advances; and on the point in question, there is no controversy which deserves the name. There is, indeed, no rule without exception. At the meeting of the British Association held last year at Liverpool, I remember an elaborate paper was published to prove that the theory of gravitation was contrary to Scripture: it, of course, called forth no remark. At Newcastle, a gentleman, well entitled from his labours in one department of the science, to be listened to with respect, more especially as he did not impugn opinions differing from his own, took what I must call the sceptical side of this enquiry, by endeavouring to prove the uncertainty of geological evidence. The paper was honoured by a reply from Professor Sedgwick, whose reasonings were responded to by an audience containing a greater amount of high geological authority, than perhaps was ever before congregated under one roof, in a manner which proved that on this point at least there was no dispute.

† M'Culloch's Western Islands, vol. ii. p. 480.

Although we have traces of changes of level on every side of the British Islands, it would be premature to say whether or not they are all universal, or whether some of them may not be confined to particular districts. There can, I apprehend, be no doubt as to the lower levels under the great terrace; the plateau at its base, except where since worn away by the action of the sea, is invariably composed of marine beds of sand, gravel, or clay; but the case is doubtful as to those at higher elevations; and if the shells at the top of the mountain of Moel Tryfane be considered as a proof of elevation, we may safely assume that it must have been a local one.* Although we do not observe any such marks of violence as are indicated by extensive inclinations of the stratification, or by the fractures, erosions, and unstratified deposits which have been produced by diluvial agency, it is quite evident that some of these changes must have been sudden; and beds of testaceous animals have been entombed alive by the subsequent deposit of clay or sand from a considerable depth. This is particularly observable in the laminated clay in which marine remains are so frequently found in the basin of the Clyde. The upper parts seem quite destitute of them, and it is only when the excavations are made deep enough, such as in digging wells or coal-pits, or in the lower beds of brick-works, that they may be ex-

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* Since writing the above, I have read with much pleasure Mr Trimmer's paper on the diluvial drift in Wales and Ireland in the Journal of the Dublin Geological Society. I agree with him entirely as to the well-marked difference between diluvial deposits and those caused by permanent submergence; and if I differ with him as to the origin of the gravels of Howth and Bray, it does not in the slightest degree affect the argument. He appears, for he has not come to that part of the subject, to consider them the result of diluvial action, whilst I agree with my friend Dr Scouler, with whom I visited them, that they are proofs of elevation. Mr Trimmer, after noticing the ready reception of the diluvial theory of Buckland, remarks, that "the interest excited by these new and striking facts (*i. e.* proofs of change of level) had now diverted the current of geological speculation into an opposite direction from that in which it had lately flowed, and from the one extreme of having generalized too hastily on diluvial phenomena, geologists began to run into the other, of endeavouring to exclude diluvial action from the list of geological agencies,—to expunge the very name from geological nomenclature,—to forget all the evidence which had been collected of the passage of large bodies of water over the land, and, in every mass of transported gravel in which marine shells of existing species were

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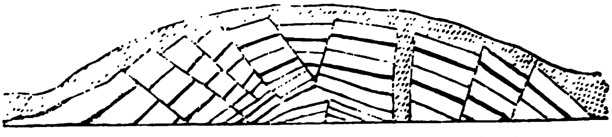
discovered, to see a raised beach, or a marine formation, of gradual accumulation, regardless of the proofs which, in many cases, existed of such deposits being due to the sudden and transient action of the sea." It is impossible to examine the diluvial deposits which I have formerly noticed, without remarking the evident effects of such sudden and transient action, so perfectly resembling those which we know must have been owing to similar causes. In the summer of 1818, I had an opportunity of observing the deposit caused by the eruption of the lake which had been formed by a glacier in the Valley of Bagne, and which was spread over the valleys of the Dranse and the Rhone before it was covered by vegetation or obliterated by cultivation. No word could so well express its appearance as diluvium, except that the occurrence of works of art formed a prominent feature, especially below the village of Martigny, where several houses were destroyed, and where beams, hewn stones, and fragments of furniture, were confusedly mixed with gravel and clay. At Greenock, in 1834, I witnessed the effects of an inundation, caused by the breaking down of the head of a reservoir, in which upwards of thirty lives were destroyed in its tract to the sea. It exhibited all the phenomena of diluvial action. The streets and walls were marked with furrows, masses of stone, and even of cast-iron, were mixed up with clay and gravel without regard to their gravity, whilst within the houses every thing was covered with a thick layer of fine silt exactly as in the diluvial caves. Were this covering, therefore, to occur in insulated patches, we might seek in similar causes for similar effects; but where could the lake have existed so vast as to have swept away nearly the whole of the alluvial covering of the great coal basin of Scotland from sea to sea, and lodged in one confused mass in some places hundreds of feet in thickness? It must, I apprehend, be sought for in some sudden geological action of a magnitude far surpassing any like event recorded in the short page of human history. The long continued action of submarine currents could not have been the cause of the beds in question, although I have no doubt that they often have given origin to coarse beds of gravel improperly termed diluvium.

pected to be found. In the brick-works near Glasgow, I am often told by the workman that they are not deep enough for shells.

Such sudden changes we know have in recent times taken place on the west coast of America, and in Cutch; and no doubt earthquakes have accompanied the ancient changes as well as those of a modern date. Fissures and dislocations are occasionally to be observed in the beds of sand and clay, produced probably by such causes. In an excavation made in the line of the Edinburgh and Leith railway, in cutting through a ridge of about 400 yards long, 100 yards broad and 10 yards high, the section, which was at right angles with its length, exhibited numerous rents traversing the beds, which could only have been produced by a sudden upheaving. A horizontal section would have represented the fissures as parallel with its length, whilst the cross one shews them radiating, as it were, from a centre. The inclination of the beds is too great to be ascribed to original inequalities in the mode of deposition. In some cases they form an angle of more than 60 degrees with the horizon. Some of them consist of fine and coarse sand or clay, and others of small fragments of coal. The section presented a beautiful miniature model of the stratification, fissures, slips, and faults of a coal-field. These beds are covered by another of gravel, which lies unconformable to them, and has evidently been deposited immediately after, filling from above some of the open fissures. It is impossible to account for these appearances, without supposing that they are the effect of a local upheaving.



Although, however, the changes in level might in some cases have been sudden and attended with earthquakes, it is probable that in others they have been slow and gradual, like those taking place in Sweden at the present day. Indeed, with the exception of the absence of works of art, nothing can more perfectly agree with the appearances of the ancient marine alluvial beds than Mr Lyell's description of similar, but more recent ones in Sweden. I have often met with beds of shells imbedded in marly clay, which had received a violet colour from the decomposition of the common mussel (*Mytilus edulis*), exactly as described by him.*

The question as to the identity of the flora and fauna of the present period, with that of submergence, is an important one. It would perhaps be premature to say with certainty, whether they are identical or not. With regard to the vegetation no observations which have yet been made shew any difference between it and the existing race of plants. But too little has been done in this department to be of any value in settling the question. The same observation applies to the remains of birds and land animals; or to those of cetaceæ, crustacea, algæ, zoophytes, and other marine remains which have been found in these deposits. I have endeavoured to institute as rigorous a comparison as I could between the testacea of the two periods, and refer to the catalogues which I have appended to this paper for the results. It will be observed that, although the greatest proportion of the shells are identical with existing species, there is a certain proportion which differs from them.† Of those which are unknown, some may probably yet be discovered in a recent state; while others, in place of being specially different from their recent congeners, may be only varieties arising from the different circumstances under which they were placed. Still, as the per-centage of unknown shells is greater than that of the newer pliocene of the Val di Noto in Sicily, it appears highly probable, therefore, that some change in the fauna must have taken place.

The organic remains belonging to these deposits have been termed Quaternery by M. Risso, and subfossil by other geologists. Professor Phillips includes the beds in which they are found amongst the post-tertiary and modern deposits,—although with some doubt; observing, that it is difficult to " discriminate between the Sicilian tertiaries with 95 per cent. of existing species of shells and the conchiferous gravels and sands of Holderness and Lancashire, in which, among twenty species of shells now living in the German Ocean, *one* occurs which is not yet known. If the Lancashire shells are, like those of Specton, Udevalla, and the coasts of Devon and Calvados, raised beaches, and to be classed in the modern epoch, why are the Sicilian ranked as tertiary?"* It appears to me that Mr Lyell has solved the difficulty, by classing amongst the tertiary formations "all those geological monuments which cannot be proved to have originated since the earth was inhabited by man." The appearance of man on the surface of the earth is an event of such transcendent importance, as to justify its being used as the separating line of the recent or human period, and those which preceded it. Changes of level have occurred in every stage of the earth's history: those of which I have been treating must have taken place during that which immediately preceded the recent period, and, of course, the organic remains belonging to that division of the tertiary group, which he has named the newer pliocene. It is of great importance that every circumstance connected with this deposit should be carefully observed and recorded, as an accurate knowledge of it cannot fail to throw much light on that hitherto obscure branch of geology, the nature and origin of the different alluvial beds which compose the earthy covering of the more ancient formations; and, as it must be the object of the science to proceed from what is known to what is unknown, we cannot too minutely investigate that part of it which forms the first step in the descending series, in order that we may obtain firmer footing in prosecuting our researches into the more remote epochs of the history of the earth.†

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* Phil. Trans. 1835, p. 1.

† At the late meeting of the British Association at Newcastle, I had an opportunity of clearing up some points of interest respecting the unknown species of shells belonging to these deposits, and have to acknowledge the advantage I derived from the kind assistance of Messrs Adamson and Alder, and from my visits to the Museum of Natural History, which is arranged in a manner well worthy of the scientific reputation of that splendid city.

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* Treatise on Geology, p. 263.

† The catalogues which accompany the memoir consist of one of the recent marine shells in that portion of the British seas which extends from the Firth of Clyde to the north coast of Ireland, containing 276 species, of which five are new to the British Fauna; and another of the fossil shells described in the paper, chiefly found in the same geographical region, containing 180 species, of which about fourteen are unknown or extinct. These catalogues, with figures and descriptions of the unknown species, will we expect appear in a future Number, accompanied, it is hoped, by a notice from M. Deshayes on the subject.

Proceedings of the Royal Society.

1838, *January* 15.—Sir T. M. Brisbane, Bart., President, in the Chair. The following Communications were read:—

1. Notice regarding the Composition of Dr James's Fever Powder. By Dr Douglas Maclagan, F. R. C. S. E.
2. Observations on the *Cysticercus Cellulosæ* inhabiting the Human Muscles. By Dr Knox.
3. Examination of certain Objections to the Theory of Isomorphism. By Henry Madden, Esq.

February 5.—Sir George Mackenzie, Bart., in the Chair. The following Communications were read.

1. Observations on the Parr and Young of the Salmon. By Dr Parnell.
2. Notice of the remarkable Mathematical Properties of a certain Parallelogram. By John Scott Russell, M. A., F. R. S. E.

February 19.—Dr Hope, V. P., in the Chair. The following Communications were read:

1. On the Composition of a new Ink. By Professor Traill.
2. Abstract of first part of Memoir on the Mid-Lothian and East Lothian Coal Districts. By David Milne, Esq.

The author commenced his communication by stating, that he should divide his memoir into two parts, the first being devoted to a mere narrative of *facts*, the second to *explanations* of these facts.

In describing the geological features of the district, he noticed first the *stratified* and next the *unstratified* rocks.

The STRATIFIED consist of sandstone, shales, limestones, coal, and clays; which rocks seemed severally to abound or prevail, in the order now stated.

These stratified rocks overspread the district from Portobello to Gladsmuir, in an east and west direction; and from the Firth of Forth to the Lammermoor Hills, in a north and south direction. Within these limits there are two basins,—the basin of the Esks, and the basin of the Tyne; and which basins are divided by the ridge or high ground that runs from Prestonpans (on the shore of the Firth) by Tranent, Falside, Carberry, and the Roman Camp.

The Esk basin contains between sixty and seventy coal-seams, exceeding one foot in thickness. The Tyne basin contains not more than ten or twelve, being those which appertain to the lower part of the deposit. The vertical depth of the Esk basin in its trough,—or, in other words, the thickness of the deposits composing it, is between 1000 and 1050 fathoms ; but this thickness diminishes rapidly to the south, arising chiefly from the coal-seams and sandstone strata thinning away in that direction ; and which diminution is compensated in but a trifling degree, by the increased thickness of the limestone in the same direction.

The Tyne basin comprehends only the deposits lying below the Great Seam of coal, and the entire thickness of these, is greatly less than in the Esk basin, arising chiefly from the sandstones and coals thinning away towards the east.

The strata on the west side of the *Esk* basin, dip to the east and south-east at a very steep angle, and are in some places absolutely vertical. On the opposite or east side of the Esk basin, they slope to the west at an angle varying from 20° to 40°.

The strata on the west side of the *Tyne* basin dip to the east, at an angle varying from 15° to 20° ; and on the opposite or east side of the same basin, they dip to the west at an angle of only 5° or 6°.

In the intervening ridge which runs from Prestonpans to the Roman Camp, the strata mantle over from the one basin into the other, and are there occasionally broken so as to shew an anticlinal line.

The different kinds of coal were described ; and it was stated, that each kind was characterized by differences in the fissures or joints intersecting them.

The principal slips in the district were next pointed out ; and the author referred to a table he had constructed, shewing the direction of each, and the amount of dislocation produced on the intersected strata. Between eighty and ninety of these were also laid down on the map accompanying the memoir.

In describing the UNSTRATIFIED rocks of the district, the author mentioned, there were no hills or amorphous masses of trap within the proper limits of the Lothian coal-field ; and that the trap shewed itself only in dykes, which run in straight lines intersecting the strata. He pointed out three or four such dykes, varying from 60 to 120 feet in width,—all of which appeared gradually to thicken towards the west, and all of which run in nearly an east and west direction.

March 5.—Sir T. M. BRISBANE, Bart. P. in the Chair.

The following Honorary Fellows were elected :

Prof. Tiedemann, Heidelberg. Prof. Müller, Göttingen.

The following Communications were made :

1. Notice regarding a New British Species of *Coregonus*. By Dr Parnell.
2. Abstract of Part II. of Memoir descriptive and explanatory of the Mid-Lothian and East Lothian Coal-fields.

This part of the memoir was devoted exclusively to an attempt to *explain* or account for the facts described in the first part of the memoir.

The geological epoch when the strata composing the coal-measures of this district were deposited, was first noticed. It was stated that these coal-measures were deposited at a period immediately following the deposition of the old red sandstone formation; and that this older formation was to be seen both on the north and on the south sides of the Lammermuir Hills, dipping under the coal-measures, and resting on the upturned greywacke strata of these hills. The lowest member of the old red sandstone group is a coarse conglomerate, evidently formed by the denudation of these greywacke hills; and the uppermost members consist of soft argillaceous sandstone, of a deep red colour.

The coal-measures of this particular district extend also over the whole of East Lothian, though towards the north and north-east, they are much broken and shattered by eruptions of trap. Beyond the east and south-east crop of the Tyne Coal Basin, there are no members of the coal-measures proper, except those which occupy the lowest part of the series. The limestone known in the Esk and the Tyne Basins, as lying under the North Greens Coal, undulates along or near the surface, throughout all the country between Haddington and the sea, towards North Berwick, as well as Dunbar and Dunglass, except at those places now occupied by trap hills.

It appears that along the east margin of the Tyne Basin there is an anticlinal line, from which the limestone just spoken of dips gently to the east, so as to form in some measure a third basin, but one very flat and much broken.

The author proceeded next to speculate on the probable mode in which the strata of the coal-measures had been severally formed. It was inferred from various circumstances, that they must have been all deposited at the bottom of an aqueous medium, which in certain parts, or at certain periods, was calm and tranquil, and which, in other parts, or at other periods, was agitated by currents.

The *shales* and *clays*, it was said, might have been formed of sediment washed down from the Lammermuir Hills; but it was thought that the siliceous sediment necessary for the enormous deposits of *sandstone* must have come, not from the greywacke hills chiefly, but from primitive formations, and which were probably situated far off towards the west and north.

In regard to the origin of the *limestone* strata in the district, it was inferred, that they had been formed by a precipitate of carbonate of lime, held in solution by the aqueous medium which overspread the district. Water, it was said, would hold carbonate of lime in solution, if there was an excess of carbonic acid; and on the application of heat, a part of the carbonic acid gas would be liberated, and a precipitate occasioned. To account for the remarkable fact, that the limestone underlying the North Greens coal thickened as it approached the Lammermuir Hills, it was suggested that the water may have been warmer near them, not only from its being shallower there, and thus more capable of being acted on by solar and atmospheric influence,—but also from the subterranean heat being there more intensely felt or more frequently evolved. The same theory might account for the formation of limestone in the upper part of the basin, though in proportion to the distance between the subterranean heat and the water, the precipitates of lime would become less abundant and less frequent; a consideration which would explain why the five or six beds of limestone that occur in the district, are all situated in the lower half of the basin.

With regard to the origin of the *coal*-seams, it was observed, there could be no doubt of their having been formed from accumulations of vegetable matter, which, at different periods, had been drifted from a distance. The fact of these accumulations having taken place at the bottom of the same sea or aqueous medium in which the other strata of the basin were successively deposited, seemed to be placed beyond dispute, by the discovery in the substance of the coal itself, and about four inches below the top of a seam of coal,—of quantities of the teeth, bones, and scales of fish. These remains belong to the same species, the remains of which had been found in the shales of this coal-field by Lord Greenock, and in the Burdiehouse limestone by Dr Hibbert.

The vegetable matter which formed most, if not all, of the coal-seams of the district, appeared to have been drifted from the west or north-west, because in these directions the seams get gradually thicker.

As all the vegetables found in the coal-seams and adjacent shales are terrestrial, it seemed probable that they had been torn off or swept away from their native sites by periodical inundations:—that they had been carried out into a great lake or estuary, where, after floating about for a considerable time, they subsided in tranquil waters, but not before parting with the clay and other earthy matters attached to their roots, and not before the sediment carried off by the inundation, and mechanically suspended in the water, had time to subside. It was inferred from this circumstance, that beneath every seam of coal there ought to be a bed of fire-clay,—an inference which agrees with the fact.

After these vegetable accumulations had taken place, and probably after all the other strata of the basin were deposited, the entire series of rocks had evidently been operated on by some powerful agent, so as to give to each its peculiar crystalline structure, and many of the other known peculiarities in their constitution. That some such agent must have operated appears to be proved, (1.) By the formation of joints and fissures intersecting the strata; (2.) By the conformity of direction in these joints and fissures; (3.) By the internal movement of the particles or ingredients of the vegetable mass among each other, whereby different kinds of coal were formed in the same seam; and (4.) By the fissures and cavities in the coal being generally filled with *pearl-spar*, which could only be derived from an aqueous medium impregnating the vegetable mass, and holding carbonate of lime and magnesia in solution. It was inferred from these and other circumstances, that the agent which had operated on the strata after their deposition, was subterranean heat.

The rest of the paper was occupied with an explanation of the convulsions which had occurred after the deposition of these strata, and in consequence of which they had been thrown into the form and position now presented by them. It was mentioned that the very steep dip to the eastward, which the strata on the west side of the Esk basin presented, was occasioned by the eruption of Arthur Seat, Blackford Hills, and the other trap masses that occur along almost the whole extent of the west side of that basin.

It was next shewn, that before the enormous quantity of volcanic matter which burst forth emerged from beneath the strata of the district, these strata must have suffered dislocations or cracks, which would account for the slips and dykes that intersect the district. It was also shewn, how these slips and dykes behoved to run generally in a direction between N. and W., and cause the dislocated strata to sink down on the north side of them, rather than on their south sides. An explanation also was offered of the invariable rule which had been found to prevail in this district as in others, that if a slip haded or sloped from the vertical, the strata were always lowest on the upper side of the slip. A reason was also assigned for the fact, that in this district, the strata are not deranged (or in other words, "thrown down" on either one side or other) by *dykes*, but only by slips.

Proceedings of the Society of Arts for Scotland.

THE Society for the Encouragement of the Useful Arts met in the Royal Institution on Wednesday the 10th January 1838, at eight o'clock P.M. Sir John Graham Dalyell, Kt., president, in the chair. The following communications were laid before the Society :—

1. Model and Description of an Improved Electric Telegraph. By Mungo Ponton, Esq., F.R.S.E., Vice-Pres. Soc. Arts. Remitted to a committee.—Dr Hunter made some interesting remarks upon an American Electric Telegraph described in Silliman's Journal.

2. Remarks on the Geographical Position of some Points on the West Coast of Scotland. By William Galbraith, teacher of Mathematics, Edinburgh, Couns. S.A.—Thanks.

3. Notice of the Precautions to be taken while using Mr Adie's Anemometer. By Edward Sang, Esq., civil engineer, F.R.S.E., M.S.A.—Thanks.

4. Notice of a Method of determining the Velocity of the Wind. By Mr Sang.—Thanks.

The following candidates were admitted as Ordinary Members, viz.:—

Richard Hunter, Esq., Bengal Civil Service, 1 Doune Terrace; Mr Walter Nicholl, teacher of Mathematics, 82 South Bridge; Mr William Todd, bookbinder, 1 Forth Street; Mr J. W. Lyon, Merchant, 85 Great King Street; David Stevenson, Esq., civil engineer, 1 Baxter's Place; Mr Charles Macgibbon, builder, 33 East Claremont Street; John Clark, Esq., M.D., Dep. Inspector-Gen. of Hospitals, 24 Heriot Row; Mr Robert Maxton, engineer, 4 Albany Street, Leith; Mr William Lithgow, painter, 60 Hanover Street; William Carmichael, Esq., late assistant-clerk of Session, 18 Castle Street; J. C. R. Duddingston, Esq., 28 St Andrew's Square; Mr James Cooper, glass-merchant, 18 Picardy Place; Charles J. Anderson, Esq., 50 Albany Street.

Jan. 24. 1838.—Mr Dunn, curator of museum, in the chair.

The following communications were laid before the Society:—

1. Specimens of an Improved Alphabet for the Blind, consisting of the capitals and lowercase combined; with remarks on the principles which regulate the tangibility of raised characters, as ascertained by experiment and experience; by Mr James Gall, junior, printer, Edinburgh, M.S.A.—A few experiments were made to shew the extraordinary tangibility of the improved Alphabet, by means of a blind boy who has only been eight months at School.—Thanks.

2. Brief Remarks on the Progress of the Useful Arts at home and abroad, with some suggestions in reference to the Society of Arts; by Dr D. B. Reid, F.R.S.E., Vice-President Society of Arts. Illustrative Specimens were exhibited.—Thanks, and a committee appointed to take the suggestions into consideration.

3. Donation—A Letter to the Directors of the Institutions for the Blind in Great Britain and Ireland; by John Alston, Esq. of Rosemount, treasurer to the Institution for the Blind at Glasgow—Hon. M.S.A. Dated 4th July 1837; from the Author.—Thanks.

4. Donation—The Gospel according to St Mathew, printed in raised letters for the use of the Blind; Printed at the Institution Press, July 1838; with a relative letter from John Alston, Esq., and specimen of new type therein referred to. Presented by Mr Alston.—Thanks.

5. Donations—The First Class-book, printed for the Blind by J. Gall; published by the Sunday School Union of London. The Gospel according to St Luke; printed by J. Gall for the British and Foreign Bible Society. Both of the above are printed in the improved Alphabet for the Blind, and presented by Mr Gall.—Thanks.

The following candidates were admitted as Ordinary Members:—

George Lorimer, builder, Keir Street, Edinburgh; George Dundas, engineer, Dundas Castle; J. B. Blyth, student of medicine, 32 Dundas Street; C. D. May, punch-cutter, 26 Clarence Street; Bindon Blood, M.R.I.A., F.R.S.E., 22 Queen Street.

Feb. 14.—Sir John Graham Dalyell, Kt., President, in the chair. The following communications were laid before the Society:—

1. Notice of a singular phenomenon connected with the rotatory motion of fluids. By Edward Sang, civil engineer and machinist, Edinburgh, F.R.S.E., M.S.A.—Thanks voted.

2. Observations relative to a model of a vessel to be propelled by oars in place of the common paddles at present in use in steam navigation. By James Kilpatrick, engineer, Musselburgh. A working model was exhibited.—Referred to a committee.

3. Drawing and Description of a grinding machine, which is used instead of a turning-lathe for giving a truly cylindrical form to the rims of pulleys and drums.

Also Drawing and Description of a machine for grinding pulleys round on the rim. By James Whitelaw, 18 Russell Street, Glasgow.—Referred to a committee.

4. Four methods of palpable printing for the use of the Blind. By Mr M'Farlane, Comrie.

5. The committee on Mr Thomas Robertson's improved vertical watch, lever do., chronometer, reported.

6. An arbitrary alphabet for the Blind. By Richard Hunter, Esq., H.E.I.C. Service, Doune Terrace.—Thanks voted.

The following candidates were admitted as ordinary members, viz.:—

Mr Hugh Morton, engineer, Leith Walk; Mr James Trotter, teacher, 10 North St David Street; Andrew Dunlop, Esq., W.S., Scotland Street; John Wilkie, Esq. of Foulden; W. F. Lindsay Carnegie, Esq. Kinblethmont; Mr Alexander Beattie, smith, 35 Canal Street; Mr Thomas Summers, merchant, 107 South Bridge.

Feb. 28.—Sir John Graham Dalyell, Kt., President in the Chair. The following communications were laid before the Society:—

1. Description (Part 1st) of the Phoroscope, an instrument for measuring Time and Velocity. By Edward Sang, civil engineer and machinist, Edinburgh, F.R.S.E., M.S.A. The instrument in an unfinished state was shewn.

2. A method of taking the Plaster of Paris from the Types after Stereotyping, without injuring the Types. By Mr George Richardson, 35 Miller Street, Glasgow.—Referred to a committee.

3. Notice of a method of Preventing the effects of Frost on Gas-meters. By Edward Sang, Esq. Kirkaldy. Communicated by Mr E. Sang, Edinburgh, M.S.A.—Thanks voted.

4. Essay on the best mode of depriving the Mucilage of the Fuci of disagreeable Taste and Odour. By Mr G. Black, College Post-Office, Edinburgh. With illustrative Specimens in Bottles.—Referred to a Committee.

5. Verbal notice of M. D'Arcet's Process for the extraction of Gelatine from Bones, and the importance of introducing it into Hospitals, and other Public Institutions in this country. By Dr D. B. Reid, F.R.S.E., V.P.S.A.—Referred to a committee.

The following candidates were admitted as ordinary members, viz.:—

His Grace James Duke of Roxburgh; David Maclagan, M.D., F.R.S.E., John Scott Moncrieff, accountant, 68 Queen Street; James Kilpatrick, engineer.

March 14.—Dr D. B. Reid, V. P., in the chair. The following communications were laid before the Society:—

1. A Shower-Bath to be used either with hot or cold water; and which can be made at a trifling expense. By Mr James Milne, brass-founder, Chalmers' Close, Edinburgh,—Couns. S. A. The bath was shewn, and Mr Milne, in the most handsome manner, agreed to furnish a model bath for the Museum; his object being, that any tinsmith having access to see the model, may thus be able to make up a bath so cheap, that even the working classes may avail themselves of it. The Secretary observed that, on visiting Mr Milne's extensive manufactory lately, he was exceedingly pleased to see a *lying hot-water bath* in preparation, for the express purpose of enabling his workmen to avail themselves of this salubrious and agreeable detergent, which promises so materially to improve both their health and comfort; and that he hoped Mr Milne's example might be followed in other large factories.

2. An arbitrary Alphabet for the Blind. By Richard Hunter, Esq., H.E.I.Co.'s Service. Thanks. Remitted to standing committee on printing for the blind.

3. Suggestions regarding a Rotatory Steam or Water Engine,—the Barometer, the Quadrant, and the Hygroscope, &c. in a letter from Mr W. C. Cunningham, Missionary to Sir David Brewster, dated Rarotogna, South Seas, Lat. 21 deg. 14 min. S.; Lon. 160 deg. W.: March 6 1837. Communicated by Principal Sir David Brewster, K. H., F. R. SS. L. & E., M. S. A. Thanks. Referred to a committee.

4. Model, Description, and Lithographic Drawing of a Patented Improvement on Ships' Anchors, to prevent the chain cable getting foul of the arm. By Mr James Allen, shipmaster, Greenock.—Thanks. Model to be placed in Museum.

5. Specimens of (French) Ornamental Paper for the use of Bookbinders,

Casemakers, &c.; also of Paper Hangings with Metallic and other grounds were exhibited and presented to the Society by Sir John Robison, K. H., Sec. R. S. E., M. S. A. The special thanks of the Society were given from the Chair to Sir John, for the donation of these very beautiful and elegant specimens (some of them priced), and for the interest he has all along felt for the improvement of the useful arts in this country, by introducing whatever is new and useful from foreign countries.

5. A new Gimblet (French) not liable to burst the wood it passes through, and not requiring to be drawn out until the hole be completed, was also exhibited by Sir John Robison. This gimblet was much admired; cuts clean, and makes a beautifully smooth circular hole. Sir John mentioned that he hoped they would be soon manufactured in this country, as he had sent patterns to some of the English tool-makers. —Thanks voted.

7. Mr Bald exhibited—1. An ancient piece of Point-work (needlework) by a Nun, and supposed to be some hundred years' old, at least in the time of the Charleses of England. Also a specimen of modern Point-work, worthy the attention of Ayrshire embroiderers, as a work for females, or ladies retired, as an occupation yielding emolument. Mr Bald also read a Dissertation upon this kind of work, sent him by two ladies. At the request of the members, Mr Bald agreed to place these specimens in the shop of Mr Blackwood, 43 George Street, for the inspection of the ladies. 2. An ancient Fan, the work of France or Italy, at least 150 years old; the drawing upon it in Indian ink is exquisite, representing the union of philosophy with religion. 3. Two Busts, cut in Ivory, from France, a male and female figure, most exquisitely and symmetrically carved. Thanks were voted to Mr Bald for the exhibition of these specimens.

8. Donation.—General Tables for computing the Obliquity of the Ecliptic; converting Mean Solar into Sidereal Time, &c. &c., with the Catalogues of Bessel and Brinkley, &c. By William Galbraith, M.A. Second Edition. Edin. 1836. From the Author.—Thanks.

9. Donation.—Barometric Tables for the use of Engineers, Geologists, and Scientific Travellers. By William Galbraith, M.A. Edinburgh, 1833. From the Author.—Thanks.

Onesiphorous Tyndall Bruce of Falkland, F.R.S.E., was admitted as an Ordinary Member. The Society adjourned to 28th March.

March 28.—Sir John Graham Dalyell, Kt., President, in the chair. The following communications were laid before the Society:—

1. On the expediency of forming national establishments for moulding and casting works of art, with observations on the improvements which would result from the adoption of such a measure. By Charles H. Wilson, Esq. R.I.A. & A.S.A. Communicated by Sir John Robison, K.H.,

Sec., R.S.E., M.S.A.—Thanks. Referred to a committee. The object of this interesting paper, was to shew the advantages that would result from the formation of such establishments in Great Britain, which, by the facility they afforded to the diffusion throughout the country of the purest models for instruction and imitation, would improve the taste and execution, not merely of artists, but of artisans and manufacturers, and at the same time raise the standard of public feeling, and create a demand for works of a superior class, in every department of the arts. The benefits which had resulted from the adoption of this plan in France were satisfactorily shewn, where the artisan, when he wants to aid his invention, can easily procure from the Louvre, at a cheap rate, such classic models as suit his purpose, and, working with these before his eyes, fresh in his recollection, or within reach of constant reference, his hand becomes accustomed to obey the eye and the imagination in producing chaste and beautiful forms with fidelity, promptitude, and facility.

After remarks from the President and several members in approbation of Mr Wilson's communication, and of the measure it proposed, a committee was appointed to consider in what manner the Society could best promote its success. The Society unanimously resolved that Mr Wilson's paper should be printed.

It appears that the formation of such establishments in this country had been suggested by the author's father, Mr Andrew Wilson, now residing in Italy, and well known as a distinguished artist and man of taste, in a communication made to the Board of Trustees here last summer. It is understood that this board, which deserves so much credit for the taste and judgment with which they have selected the admirable collection of casts in the gallery of the Institution, and which they have opened with so much liberality to the public, are taking a warm interest in the promotion of the measures.

2. A Substitute for a Mortoise Lock was exhibited, being an improved specimen of a construction which was formerly shewn to the Society. By Sir John Robison. Thanks.

3. On a mode of depriving the Mucilage of Lichens of disagreeable taste and odour. By Mr G. Black, College Post-Office, Edinburgh. With illustrative specimens in phials. Referred to a committee.

4. Description of an improved Nut for leading screws. By Edward Sang, civil-engineer and machinist, Edinburgh, F.R.S.E., M.S.A. The nut and drawings were exhibited. Referred to a committee.

5. Letter on the subject of Alphabets for the Blind; with specimens of an alphabet, the letters of which are raised with a stilletto, which she has employed in teaching the blind to read. By Miss Margaret Banks, formerly of Newington, Edinburgh, now at Bewdley.—Thanks. Remitted to the standing committee.

6. Donation.—The eleventh Report by the Directors of the Asylum for the Blind, Glasgow; dated 15th January 1838. Presented by John Alton, Esq. of Rosemount, Hon. M.S.A.—Thanks.

7. Donation.—Report by the Directors of the Edinburgh Society for the industrious Blind of Scotland for 1837. Presented by D. J. Macbraire, Esq. Thanks.

The following candidates were admitted as Ordinary Members:—

The Right Hon. James Forrest of Comiston, Lord Provost of the City of Edinburgh; Thomas Henderson, Esq. of Press, 11 Melville Crescent; Eagle Henderson, Esq. Coates Crescent; Mr Lawrence Hyslop, merchant, Stockbridge; Mr John Low, plumber, 39 Lothian Street.

April 11.—Sir John Graham Dalyell, Kt., President, in the chair. The following communications were laid before the Society:—

1. On a formula to obtain the Decrease of Temperature according to the height above the earth's surface. By William Galbraith, M. A.—Couns. S.A., teacher of Mathematics, Edinburgh. (546.) Thanks.

2. Description of a Method of making a good copying Ink. By Mr J. Scott, 9 Bank Street, Edinburgh. With a specimen bottle, and impression. (540.) Referred to a committee.

3. A specific prize (the 4th) having been offered for "A convenient mode of filling the Boilers of Steam-vessels with water while the vessel is at rest, so as to remove a frequent cause of explosion;" the following communications were received and read, viz.—

(1.) Mr N. P. Comins, Madeira Place, North Leith. (474.)
(2.) Mr Andrew Carrick, Calton, Glasgow. With a drawing. (506.)
(3.) Mr Robert Mudie, 14 Fountainbridge, Edinburgh. With a drawing. (509.)
(4.) Mr John Combe, late of Edinburgh, presently in Leeds. With a drawing. (529.)
(5.) Mr John Rose, North Leith. With a drawing. (537.)
(6.) John Scott Russell, M.A., F.R.S.E. With a drawing. (Not competing.) (543.)
(7.) Mr James Gall jun., printer, Edinburgh, M.S.A. With a drawing. (548.)
(8.) Robert Bald, Esq. F.R.S.E., M.S.A. With a drawing. (549.)

All referred to a committee.

The following candidates were admitted as Ordinary Members:—

George Watson, Esq., Secretary National Bank of Scotland; Charles H. Wilson, Esq., A.S. Acad., Artist, York Place, Edinburgh.

April 25.—Sir John Graham Dalyell, Kt., President, in the chair. The following communications were laid before the Society:—

1. Mr Beattie exhibited two specimens of Two-way Door-Springs, on the construction formerly submitted by him, but in which the improvement suggested by the Committee of the Society of Arts has been adopted, along with some further ones. (547.) Sir John Robison explained these improvements, and Mr Beattie and he received the thanks of the Society.

2. Notice of a Dioptric Light erected at Kirkaldy; with Description of Apparatus used for giving the true Optical Curve. By Edward Sang, F.R.S.E., M.S.A., civil-engineer, Edinburgh. The apparatus was exhibited. (552.) Referred to a committee.

3. Description and Drawing of a Plan for keeping the Boilers of High-pressure Steam-engines always filled with water up to the required level, both when the engines are working and when they are not at work; together with Remarks as to how the plan may be applied to the Boilers of Condensing Steam-engines. This method of feeding boilers will, it is said, also answer in a steam-boat, although it is only used, in this case, to keep plenty of water in the boiler at any time the steam is up, and the engine not in motion. By Mr James Whitelaw, engineer, late of Glasgow, now 7 King's Terrace, Bagnigge Wells Road, London. (553.) Referred to a committee.

4. Mr Bald exhibited a specimen of Iron-wire Rope sent to him lately from the Fahlun Copper Mines, Sweden, each link of which is fifty fathoms, or 300 feet, in length. (554.) Thanks.

5. Mr Bald also exhibited a Pump Bucket, or Valve, from Sweden,—so constructed that the pressure on the stuffing or leather is in proportion to the column of water lifted; the principle being applicable in a variety of cases, particularly in engine-pits. Mr Bald contrasted this Swedish Valve with those presently in use in Scotland, of which he exhibited specimens. (545.) Thanks. Mr Bald, at the request of the Society, agreed to put his remarks on these two subjects in writing, and to furnish a drawing of the Swedish Valve, to be engraved for the Society's Transactions.

6. The Committee on Mr C. H. Wilson's paper on the Expediency of forming National Establishments for the Moulding and Casting of Works of Art (Dr Maclagan, convener) gave in their report, which was read and approved of. A Memorial to Government, prepared by the Committee, and founded on the report, was also read and approved of, and remitted to a committee to carry it into effect. (535.)

7. Donation—The Suburban Gardener and Villa Companion. By J. C. Loudon, Esq. F.L.S., Hon. M.S.A., &c. From the Author. (471.) Thanks.

8. Donation—Description of a Conical and Grooved Pulverising Land Roller. By J. S. Hepburn, Esq. of Colquhalzie, M.S.A. From Transactions of Highland Society, vol. xi. Presented by the Author. (498.) Thanks.

Mr Robert Caldwell, builder, Edinburgh, was admitted as an ordinary member.

May 16.—Sir John Graham Dalyell, Kt., President, in the chair.—Sir John stated, that certain letters on the present defective and oppressive state of the Law of Patent for Inventions had been addressed to him as President of the Society; but the omission of his correspondent, a very intelligent person, to specify his place of abode, had hitherto precluded him from communications in return. As nothing could be more desirable to those who valued truth and expediency than being guided to both through whatever channel, he should now propose the appointment of a special committee to investigate the subject.—Agreed to.

The following communications were then laid before the Society:—

1. On the best Method of Constructing the Interior of Buildings intended for the accommodation of Auditors and Spectators, and its application to Lecture-rooms, Churches, and Music-rooms, by arranging the Seating according to certain Isacoustic and Iseidomal Curves. By John Scott Russell, M.A., F.R.S.E., M.S.A. Drawings were exhibited. (542.) Thanks. To be printed.

2. Mr Sang concluded his paper on the Construction of Oblique Arches. Thanks. To be printed.

3. Description and Sketch of a new Instrument for sharpening and setting the Teeth of Saws. By Mr Alexander Proudfoot, Newbigging, near Carnwath. (541.) Referred to a committee.

4. Model and Description of an Improved Bottling Cock, suitable for wines, ales, porter, &c. By Mr R. Rettie, brassfoundry, 20 Royal Exchange Square, Glasgow. (502.) Referred to a committee.

5. Drawing and Description of a proposed Rotatory Steam-engine. By Mr J. B. Montrose. (558.) Referred to a committee.

6. A Musical Catechism, with Tunes for the use of the Blind. Printed in Raised Characters at the Glasgow Asylum, 1838. By John Alston, Esq. of Rosemount, Honorary Treasurer to that Institution, Hon. M.S.A. (559.) Referred to a committee.

7. Letter on Moulding and Casting Works of Art. By Nicholas P. Comins, Esq. Madeira Place, Leith. (564.) Thanks.

8. Donation.—A Multiplication Table for the Blind. Printed in raised Characters at the Glasgow Asylum, 1838. Presented by John Alston, Esq. (560.) Thanks.

9. Donation.—Lehrbuch zum Unterrichte der Blinden. By Johann Wilhelm Klein, Director of the Institution for the Blind at Vienna; with 6 copperplates. Printed at Vienna, 1819. Presented by John Alston, Esq. (561.) Thanks.

10. Donation.—The Gospel according to St John, printed in Embossed

Stenographic Characters for the use of the Blind. Edited by Mr T. M. Lucas of Bristol. Embossed at Bristol, July 1837, and published by the Bristol Society for Embossing and Circulating the authorized version of the Bible. Presented by that Society, per Mr F. W. B. Reid, the Secretary. (562.) Thanks.

11. Donation.—Instructions for teaching the Blind to read with the Britannic or Universal Alphabet, and Embossing their Lessons, &c. By T. M. Lucas, inventor and teacher of the system. Printed at Bristol, June 1837. Presented by the Bristol Society for Embossing, &c. the Bible for the Blind. (563.) Thanks.

Mr John Adie, optician, Edinburgh, was admitted as an ordinary member.

May 30.—Mungo Ponton, Esq. F.R.S.E., Vice-President, in the chair.—The following communications were laid before the Society :—

1. On the "Principles of Propelling Vessels by Machinery." By John Scott Russell, M.A., F.R.S.E., Couns. S.A. Thanks. To be printed. (551.)

2. A Specific Prize (the 7th) having been offered for "A convenient and simple method of increasing or diminishing the distance of the floats from the centre of the common paddle-wheels, during the motion of the vessel, so as to adapt them to changes in the load and draft of water;" there had been received, and were read, the following communications, viz. from

(1.) Mr Nicholas P. Comins, Madeira Place, North Leith. With a drawing. (475.)

(2.) Mr Dixon Vallance, Libberton, Lanarkshire. (488.)

(3.) Mr John Henderson, South Bridge, Cupar-Fife. With a model. (510.)

(4.) Mr George Wilson, 3 Hanover Street, Edinburgh. With model and drawing. (534.)

(5.) Mr John Rose, 2 Great Junction Street, North Leith. With a drawing. (538.)

(6.) Mr Andrew Black, watchmaker, Leslie, by Markinch. With a drawing. (550.) All referred to a committee.

3. Drawing and Description of a "New Method of constructing a Paddle-wheel" for Steam-vessels. By Mr Andrew Carrick, Calton, Glasgow. (507.) Referred to a committee.

4. Drawing and Description of a "Hydrostatic Safety-valve for Steam-boilers." By the same. (505.) Referred to a committee.

5. A Specific Prize (the 10th) having been offered "For the best and simplest method of ascertaining the quantity of absolute alcohol in any mixture." The following communication having been received, was read, viz. from Mr Nicholas P. Comins, Madeira Place, North Leith. (478.) Referred to a committee.

6. Model, Description, and Drawing of a "Sink-box for preventing offensive effluvia from sinks and sewers. By Mr R. Rettie, brassfoundry, 20 Royal Exchange Square, Glasgow. One of the sink-boxes of the full size was also exhibited. (503.) Referred to a committee.

7. The Committee on Mr Rettie's "Bottling Cock" reported. Read and approved.

8. The Committee on Mr J. Scott's "Copying ink" reported. Read and approved.

Patrick Wilson, Esq. architect, 15 St Andrew's Square, was admitted as an ordinary member.

The Report of the Committee appointed to regulate the details connected with the formation of an Experimental Committee was taken into consideration, and, after some alteration, was approved of, and the Rules ordered to be printed.

June 26.—Dr D. B. Reid, F.R.S.E., Vice-President, in the chair.—The Secretary read a letter from Lord John Russell, saying he had presented the Address of this Society to her Majesty the Queen on her accession to the Throne, and that it had been very graciously received by her Majesty.

The Address is of the following tenor:—

"Unto her Most Gracious Majesty the Queen,

"Most Gracious Sovereign,

"We, the Society for Encouragement of the Useful Arts in Scotland, humbly approach your august person, to offer our ardent congratulations on your Majesty's accession to the Throne.

"From the fruit of those transcendent virtues already adorning your Royal mind, we confidently augur, in this auspicious event, increasing honour and glory to the British empire;—that its destinies shall flourish under your Majesty's dominion;—that it shall preserve its proud pre-eminence amidst the most illustrious of nations.

"Through the medium of our Institution, we seek to foster industry, and to reward distinguished merit, by encouraging inventions or improvements of the useful arts. In these we behold the elements of trade and commerce, the sources of civilization, of wealth and independence; and in teaching the benefit of interesting pursuits, we find the harbingers of tranquillity, content, and comfort;—thus strengthening the pillars of the state, and contributing to the firmest foundation of national prosperity.

"We humbly implore the Divine protection of your Majesty in the enjoyment of a peaceful and a happy reign;—that you may be long spared for the profound reverence and the unabating affections of your loyal people.

"Signed in our name, and by our appointment, by the President,
Vice-Presidents, Secretary, and Treasurer,—at Edinburgh, in
the year of our Lord, 1837, on the 6th day of December.
(Signed) "JOHN GRAHAM DALYELL, President.
"DAVID BOSWELL REID, Vice-President.
"M. PONTON, Vice-President.
"JAMES TOD, Secretary.
(L.S.) "GILBERT MARJORIBANKS, Treasurer."

The following communications were laid before the Society :

1. Mr Ponton's Electric Telegraph, in an improved form, was exhibited in action, and a brief explanation of its principles was read, after which Mr Ponton presented his Model to the Society, to be placed in their Museum. (492.)—Thanks.

2. On the Adulteration of Fixed Oils. By William Davidson, M.D., West Nile Street, Glasgow. Specimens were exhibited. (554.)

3. On the Decolorization of Palm Oil. By the same. Specimens were exhibited. (555.)

4. On the Removal of the Fetid Odour of Fish Oils. By the same. (556.)

5. On the Removal of the Bitter Taste and Lichenous Odour of Iceland Moss. By the same. (557.)

These four papers by Mr Davidson were referred to a committee.

6. Note on Ventilation ; being a sequel or appendix to a former paper on that subject. By J. Stewart Hepburn, Esq. of Colquhalzie. (567.) Thanks.

7. Specimens of a Series of Fables, with Woodcut Illustrations, now in progress of being printed for the Blind at the Glasgow Asylum. By John Alston, Esq. of Rosemount, Hon. M.S.A. (568.) Referred to a committee.

8. Donation.—Researches on Heat. Third Series. 1st, On the unequally Polarizable Nature of the different kinds of Heat. 2d, On the Depolarization of Heat. 3d, On the Refrangibility of Heat. By Jas. D. Forbes, Esq. F.R.SS. L. and E., F.G.S., Professor of Natural Philosophy in the University of Edinburgh, M.S.A. From the Transactions of the Royal Society of Edinburgh. 1838. From the Author. (569.) Thanks.

9. Donation.—Observations on the Employment, Education, and Habits of the Blind; with a comparative view of the benefits of the Asylum and School Systems. By Mr Thomas Anderson, late Manager of the Asylum for the Blind, Edinburgh, and Master of the Wilberforce Memorial School for the Blind, York (now at Manchester). Printed in 1837. From the Author, per D. J. Macbraire, Esq. (566.) Thanks.

10. Donation.—A method, by means of a Time-keeper, for finding Greenwich Time, and Latitude and Longitude, at any place by sea or land. By Mr John Johnstone. Printed at London, 1838; with two Lithographic Plates. (565.) Thanks.

1. The following candidates were admitted as ordinary members, viz.:—

Mr Robert Grant, bookseller, 82 Prince's Street; Mr Alex. G. Wighton, jeweller, 24 James's Square; James Malcolm, Esq. S.S.C. 30 Dundas Street; Mr William Phillip, watchmaker, 48 Hanover Street; Lieut.-Col. Tho. Blanshard, Royal Engineers, 135 George Street; Mr K. Mathieson jun. contractor, Glasgow, presently at Buckhaven Harbour Works.

2. The rules and regulations connected with the formation and proceedings of the Experimental Committee approved of by the Society at last meeting, were circulated to the members of the Society.

3. *Museum.*—The Curator announced to the members, that they and their friends should have access to the Museum, during the summer, on Saturdays, from ten to twelve o'clock, A.M.

July 18.—Sir John Graham Dalyell, Kt. President, in the chair. The following communications were laid before the Society:—

1. Mr Macpherson (of Smiths', Blair Street) exhibited in operation a simple Washing Machine, on a construction used in America, and believed to be originally from Russia, which is found to save time and fatigue, and to do the work much more efficiently. (573.) Thanks from the chair.

2. Donation.—Elemens de Lecture ou Exercices Syllabiques à l'usage des jeunes aveugles, &c. Par M. Guillié. Paris, 1820. Presented by Mr George Hay, brother of the late Mr Alexander Hay, author of Papers on an Alphabet and method of printing for the Blind, submitted to the Society in the year 1832–33. (570.) Thanks.

3. Donation.—A Chinese Hat and Pea-Jacket, such as are worn by the Steersman in the Chinese Junks. Presented by Mr J. W. Lyon, M.S.A. (571.) Thanks.

4. The following reports of committees were read and approved of, viz.:—

(1.) On Mr Alexander's Electro-Magnetic Telegraph. (489.) And on Mr Ponton's Improved Electric Telegraph. (492.)

(2.) On Mr J. Kilpatrick's Method of applying Oars instead of Paddles to Steam-Vessels. (496.)

(3.) On Mr J. Whitelaw's Grinding Machine. (512.) To be printed.

(4.) On Mr Richardson's Method of taking Plaster of Paris from Types after Stereotyping. (516.)

(5.) On Mr Black's Paper on the Mucilage of the Fuci. (500.)

(6.) On Mr Cunningham's Rotatory Steam-Engine, Barometer, Quadrant, and Hygroscope. (530.)

(7.) On Mr Black's Paper on the Mucilage of Lichens. (501.)
(8.) On Mr Sang's Nut for Leading Screws. (418.)
(9.) On Mr Sang's Dioptric Light erected at Kirkaldy. (552.)
(10.) On Mr Beattie's Rotary Steam-Engine. (558.)
(11.) On Mr Alston's Musical Catechism for the Blind. (559.)
(12.) On, 1st, Mr Comins' Floats for Paddle-Wheels. (475.)
2d, Mr Vallance's ditto. (488.)
3d, Mr Henderson's ditto. (510.)
4th, Mr Wilson's ditto. (534.)
5th, Mr Rose's ditto. (538.)
6th, Mr Black's ditto. (550.)
(13.) On Mr Carrick's New Paddle-Wheel. (507.)
(14.) On Mr Carrick's Hydrostatic Safety-Valve. (505.)
(15.) On Mr Rettie's Sink-Box. (503.)
(16.) On Mr Alston's Fables, with Wood-cuts for the Blind. (568.)

1. The following candidates were admitted as ordinary members, viz.:—

Mr James Adam jun. engineer, 5 Scotland Street; the Rev. Tho. Monro, A.M., George Watson's Hospital.

2. The report of the Committee appointed to prepare a List of the Prizes to be offered for Session 1838–39, was read and approved of, and the list was ordered to be immediately printed and advertised as usual.

Aug. 1.—Mungo Ponton, Esq. F.R.S.E., Vice-President, in the chair. The following communications were laid before the Society:—

1. The report of Committee.
(1.) On, 1st, Mr Comin's Boiler-Feeder. (474.)
2d, Mr Carrick's ditto. (506.)
3d, Mr Mudie's ditto. (509.)
4th, Mr Combe's ditto. (529.)
5th, Mr Rose's ditto. (573.)
6th, Mr Russell's ditto. (543.)
7th, Mr Gall's ditto. (548.)
8th, Mr Bald's ditto. (549.)
Mr W. Steele, Convener.—Not being ready, was remitted to Prize Committee when lodged.
(2.) On Mr Whitelaw's Boiler-Feeder. Mr W. Steele, Convener. (533.) Not being ready, was remitted to Prize Committee when lodged.
(3.) On Mr Proudfoot's Mode of Sharpening and Setting the Teeth of Saws. Mr P. Steele, Convener, (541.) Not being ready, was remitted to Prize Committee when lodged.

(4.) On Mr Comins' paper on determining the quantity of absolute Alcohol in any Mixture. Dr Fyfe, Convener. (478.) Read and approved.

(5.) On Dr Davidson's Paper on Adulteration of Fixed Oils. Mr Ponton, Convener. (554.)

(6.) On Dr Davidson's Paper on Decolorization of Palm Oil. Mr Ponton, Convener. (555.)

(7.) On Dr Davidson's Paper on removing the fetid Odour of Fish Oil. Mr Ponton, Convener. (556.)

(8.) On Dr Davidson's Paper on removing the Lichenous Taste and Odour from Iceland Moss. Mr Ponton, Convener. (557.)—These four papers by Dr Davidson were remitted to the Experimental Committee—Mr Ponton, Convener,—to report to Prize Committee.

2. Donation.—A Mute Converser, to enable the Seeing to communicate with the Deaf and Dumb. By W. H. Villiers Sankey, Esq. Newington, Edinburgh. (574.) Thanks voted.

1. The following candidates were elected ordinary members:—

J. B. Gallie, merchant, Gray Street, Newington; Lieutenant-Colonel James Ferguson, Hon. E.I.C.S., 53 Frederick Street.

2. Copies of the list of Prizes offered for session 1838-39, were distributed to the members.

3. The laws of the Society, as altered by the Committee, revised by the Council, and approved of by the Society at last meeting (18th July 1838), were again taken into consideration at this meeting in terms of Law XVII., and were finally considered and adopted.

After an appropriate address from the Vice-President at the close of a session of unprecedented length and interest, and during which no fewer than fifty-five new members had joined the Society, the Vice-President adjourned the Society till next session, which commences on Wednesday, 14th November next.

List of Prizes offered by the Society for the Encouragement of the Useful Arts in Scotland for Session 1838–9.

The Society for the Encouragement of the Useful Arts in Scotland, announce the following Prizes for the Session 1838–9.

I. For the most important Invention, Discovery, or Improvement in the Useful Arts;—*The* KEITH *Medal, value Twenty Sovereigns.*

II. The Society proposes also to expend the Sum of EIGHTY POUNDS, in various Pecuniary Prizes, and Honorary Medals, in rewarding approved Communications on the subjects hereafter mentioned,—and likewise on any other Inventions or Improvements which may be submitted to them,—and for Essays or detailed Accounts of Public or other undertakings of great National importance, not previously published.

1. For the best series of Experiments applicable to the Useful Arts.
2. For the most important Communication of any useful Invention, Process, or Practice from foreign countries, not yet known or adopted in Great Britain.
3. For an improvement in the methods used in Great Britain for Currying and Tawing certain kinds of Leather, so as to render them equal to those done in France.
4. For the best method of burning Gas for the purpose of *illumination.*
5. For the best means of burning Gas for supplying *heat.*

General Observations.—All communications shall be entitled to compete for the KEITH MEDAL which comply with the terms of the announcement of that Prize, although falling under any of the the above specified subjects.

The descriptions of the various inventions, &c. to be *full* and *distinct*, and, when necessary, accompanied by *Specimens*, *Drawings*, or *Models*.

The Society shall be at liberty to publish in their Transactions copies or abstracts of all Papers submitted to them. All Models, Drawings, &c. for which Prizes shall be given, shall be held to be the property of the Society; and these, and all others which shall be approved of, shall be entitled to a place in the MUSEUM.

All communications must be written on *Foolscap* paper, leaving margins at least one inch broad, so as to allow of thelr being afterwards bound up with others; and all Drawings must be on *Imperial* Drawing Paper, unless a larger sheet be requisite.

All communications to be lodged *as soon after* 1*st November* 1838 *as possible*, in order to insure their being read during the Session; *but those which cannot be lodged so early, will be received till* 1*st March* 1839.

Communications to be addressed to the SECRETARY, at the MUSEUM of the SOCIETY OF ARTS, 63 Hanover Street, Edinburgh.

ROYAL INSTITUTION, EDINBURGH, 18*th July* 1838.

Proceedings of the Botanical Society of Edinburgh.[*]

April 12. 1838.—ROBERT MAUGHAN, Esq. Member of the Wernerian Society, in the Chair.

Mr Forbes read a paper on the specific claims of *Primula acaulis, veris* and *elatior.*

Professor Christison presented some observations on the preservation of fruits and other botanical specimens in the moist state.

Mr Hamilton read a paper on the Gardens of the Ancient Hebrews.

May 10.—Professor GRAHAM, President, in the Chair.

Mr Macaulay read the first part of a Paper " On the effects of Vegetation on the Atmosphere."

Dr Graham read a description of *Catasetum discolor,* var. *luteo-aurantiacum,* and offered some general observations on the genus Catasetum.

June 14.—Dr BALFOUR, V.P., in the Chair.

The Secretary stated that a letter had been received by the President from William Gibson-Craig, Esq. M.P., inclosing a communication from Lord John Russell, intimating that Her Majesty had been graciously pleased to become Patroness of the Botanical Society.

Dr Balfour then read a paper by W. B. Carpenter, Esq. of Bristol, containing a general view of the Function of Reproduction in Vegetables.

July 12.—Professor GRAHAM, President, in the Chair.

Mr Falconer read an account of a Botanical excursion to one of the Islands of Hyeres, by Mr Percy in the year 1836, with a list of most of the species observed

Mr Macaulay read some observations on several of the species of the genus *Tortula,* communicated by Mr Robert Stark of Cirencester.

Mr Brand read a paper, containing his views on the proper mode of arranging the Society's Herbarium, and forming a catalogue for reference.

The Society then adjourned till Thursday the 8th of November.

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[*] We have refrained, in our report, giving particulars, as we are happy to find that the first volume of the Transactions of the Botanical Society, containing these, will appear about the same time with this number of the Philosophical Journal.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Temperature of the Globe in the Polar Regions and upon the tops of Lofty Mountains.*—In our climates the mean temperature of caves, wells, and common springs, very nearly corresponds with the mean temperature of the place where they are situated, as determined by the thermometer placed in the shade in the open air. This, however, no longer holds true in certain countries near the pole, and in all places near the limit of perpetual snow. There, as the observations of *Messrs Wahlenberg* and *Leopold de Buch* have especially proved, the temperature of the soil, and consequently the temperature of the springs, are notably higher than the mean temperature of the atmosphere.

This anomaly had been explained in a manner which was apparently satisfactory. The thick bed of snow which, in the northern regions, or in those whose height above the horizon is considerable, covering the soil during a considerable part of the year, cannot fail, it is said, on account of its small conducting power, to prevent the extreme cold of winter from penetrating into the earth, or at all events from propagating itself there to such depths as it would have reached, if the surface had not been covered over with such an envelope. The snow, in short, however strange the result may at first appear, is, all things considered, a cause of real heating in those regions where it remains for a long time.

Can any thing, it may be inquired, be opposed to an explanation so rational and evident? There may. For first, there is a great want of express specification. And again, since the recent epoch when *M. Erman* communicated to the Academy, his concordant comparative observations concerning the temperature of the air and of the earth, made in *Siberia*, we have, moreover, to oppose to the same explanation, that it leads, as a matter of necessity, to differences of sensible caloric for places where no such differences exist, as, for example, at Yakutsk, as we have lately learned. Those of the Scientific Commission who propose to winter near the northern extremity of Europe, may hope, therefore, to resolve an important problem in meteorology. If they remain in Finmark, at Kielvik, at Hammerfest, or at Alten, whose mean temperature is below the freezing point, they ought to investigate why water never freezes in well closed caves? The stream of Hammerfest, which, according to *M. de Buch*, never ceases to flow in the midst of winter, will also engage their attention; finally, they will not fail, were it only by making use of the simple holes which are made by the miners' borer, to examine how the temperature of the earth varies daily at different depths. These observations, I believe, have never been made in regions where, for whole months, the sun never sets. Thus, these observations will be an interesting acquisition for science, independently of their possible connection with the anomaly in the terrestrial temperatures, to which I have especially wished exclusively to devote these remarks.—*Arago.*[*]

2. *Depth of the Frozen Ground in Siberia.*—At page 435 of vol. 24th of Edinburgh New Philosophical Journal, we inserted Professor Baer's communication to the Geographical Society of London, "*On the Frozen Soil of Siberia.*" We have now to add, that a further communication on this subject, also by M. Baer, was read at the meeting of the British Association at Newcastle. After stating very shortly the nature of the experiments to be made at Yakutsk by order of the Petersburgh Academy of Science, he remarks, "It seems to me very important for physical geography, to ascertain the thickness of perpetually frozen ground in countries whose mean temperature is considerably below 0° R. I will merely mention one point: if, as is the case at Yakutsk, the ground never thaws at the depth of from 300 feet to 400 feet, all the small streams whose superficial waters only are kept in a fluid state in the summer, must be in the winter entirely without water; and, *vice versa*, we may conclude that all rivers which do not come far from the south, and whose course is entirely within those countries which preserve perpetual ground-ice, and yet do not cease to flow in winter, must receive their waters from greater depths than those which remain in a frozen state. It is, then, clear that these veins of water penetrate the perpetual ground-ice. This cir-

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[*] The notices with signature *Arago* were drawn up by that philosopher for the use of the French Scientific Expeditions, at present in the north of Europe, and in Northern Africa.

cumstance strikes me as one not devoid of interest in the theory of the formation of springs; and it would be very desirable that some researches upon this subject should be set on foot in high northern latitudes. In the narrative of Admiral Wrangel's Travels, still in MS., there occurs a remarkable instance of very considerable rivers in very cold countries being without water in winter, like our ditches and small brooks. He was riding, to the north of Yakutsk, in about 65° lat. over the ice of a large river, when the ice suddenly gave way, and his horse went under. He was himself saved by being thrown upon the ice at the moment his horse fell. He was lamenting the loss of his horse to the Yakutskis who accompanied him, as he knew not where to get another, when they laughed, and assured him they would soon get his horse back, and with a dry skin too! They got some poles and broke away the ice, under which the bed of the river was perfectly dry, as well as the horse and his saddle. The Yakutskis were therefore aware that there was no water in the winter-time at the bottom of rivers of this size; and in this case the water must have disappeared before the ice had gained sufficient thickness to bear a loaded horse. Similar accidents, and similar results must doubtless have frequently occurred, during the many journeys which the English have made in North America; and the agents of the Hudson's Bay Company must be well acquainted with the real state of the small rivers in winter, in these high latitudes; *i. e.* whether all of them are in a fluid state below the ice or not. I am collecting materials to ascertain the *southern* limit of perpetual ground-ice in the Old World; those I have are not yet very complete; but I am already aware that this phenomenon extends much farther in a southerly direction in Siberia than in Europe. The farther we go east, the more southerly we find the limit of perpetual ground-ice. Humboldt found at Bogoslovsk, in lat. 59° 45′ N., at the eastern foot of the Ural Mountains, small pieces of ice at the depth of six feet in the summer. No permanent ice has been found in Tobolsk in 58° N.; but at Beresow, in 64° N. where Erman found the temperature of the ground above +1°R. at the depth of 23 feet, we learn from the observations of M. Belowski, that the lower districts are never without ice in the ground, so that it is probable that Beresow is near the limit of perpetual ground-ice. Farther east, this frozen soil extends much more to the southward, even to the shores of the lake Baikal; indeed, the whole of the south-eastern angle of Siberia has perpetual ground-ice.* Captain Frehre states that, in 1836, he there found the ground frozen at some distance below the surface, and that this frozen stratum was continued uninterruptedly quite to the underlying rock, to a depth of from 10 feet to 40 feet. But, as he always found rock there, it would be difficult to say how thick the layer of frozen mud would be in the lat. of 52°. It thawed on the surface of the banks of the river, to a depth of from 2½ feet to 6 feet, and from 6 feet to 9 feet on the naked heights; but, in the forests, where the rays of the sun were intercepted, the thaw reached only from ¾ foot to 1 foot deep. If it be true that there are places in forests where the ground is never thawed one foot deep, it would demonstrate how little is necessary for the ground to be thawed for trees to grow on it. The development of the leaves and vegetation depends less on the temperature of the soil than on that of the air in the spring; it only requires that the ground should be so far thawed that the tree may be able to draw from it a sufficient quantity of moisture for its growth.*"

3. *On the Aurora Borealis.*—When in our climates, the aurora borealis is complete,—when one part of its light pictures on space *a well-defined arch, the culminating point of* THIS ARCH *is in the magnetic meridian,* and its two points of apparent intersection with the horizon are at equal angular distances from the same meridian.

When it projects luminous columns from different portions of the arch, *their point of intersection,* called by certain meteorologists the *centre of the cupola,* is found in the magnetic meridian, and *precisely upon the prolongation of the dipping needle.*

It is very important to repeat these kind of observations; and less with the object of establishing between the aurora borealis and terrestrial magnetism a general connection, which no one now doubts, than with the view of supplying additional information concerning the intimate nature of the phenomenon, and the geometric methods by which its absolute height has sometimes been determined.

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* Mr De la Beche remarked, that, considered geologically, this paper of Prof. Baer was an important one. It shewed that the temperature of those regions had changed since the deposit of the detrital matter (for that was the character of the frozen ground), inasmuch as, under the condition of a perpetually frozen surface, no such deposits could take place.

These methods, founded upon the combinations of parallaxes, suppose that every where the same arc is seen, by which I mean the same material particles congregated into a radiating condition by unknown causes. This hypothesis, unless I am deceived, when examined with becoming scrupulosity, will be found to be beset with many well-grounded difficulties.

The magnetic bearing of *the aurora borealis arch* proves nothing more than that the phenomenon is placed symmetrically in relation to the magnetic axis of the globe. As to the kind of displacement which the *centre of the cupola* undergoes for every change of the observer's position, it cannot be explained by the play of the parallaxes This displacement is such that an observer who proceeds from *Paris* towards the north magnetic pole, sees the centre of the cupola, situated to the south of his zenith, *elevate itself* more and more above the horizon ; but precisely the contrary would happen if the cupola were a radiating point, and not a simple effect of perspective.

So soon as it is established that in the aurora borealis, one of the portions at least is a mere illusion, we do not perceive why we are at once to adopt the notion that the luminous arc of *Paris* is that which will be perceived at *Strasburg*, at *Munich*, *Vienna*, &c. And what an immense step would be made in the theory of these mysterious phenomena if it were established that each observer sees, as it were, his own aurora borealis, as each one perceives his own rainbow ? And what would this be else than freeing our meteorological catalogues of a multitude of the determinations of elevation which cannot be considered as having any real foundation, though they have for their authors such men as *Mairan*, *Halley*, *Krafft*, *Cavendish*, and *Dalton*.

Before bringing this article to a close, in which the question of the absolute height of the matter in the midst of which the aurora borealis is formed has been introduced, I ought not to omit to state, that on one occasion Captain Parry had the conviction that he witnessed the luminous jets proceeding from the aurora projecting themselves upon a mountain which was at no great distance from his vessel. This observation well merits being confirmed and repeated.—*Arago.*

4. *On Winds.*—The variations in the wind may supply meteorologists who are travelling, by land or by sea, many subjects of the deepest interest.

First of all it is important to ascertain concerning every place, what is the direction of its prevailing winds, and what the different epochs of the year during which each wind is most common.

None of our meteorological instruments measures the velocity of the wind with any thing like desirable accuracy. When the sky is quite overcast, the observer who wishes to ascertain the rapidity of the progress of a tempest, finds himself reduced to the necessity of throwing light substances into the air, and with his watch in hand, following them with the eye till they attain different objects, the distances of which are known. When the sky is only partially covered with clouds, the course of their shadow upon the earth, for ten seconds for example, will give a very close approximation to the distances they have been moved in that time by the power of the wind.

The observation of these shadows, then, may be recommended with confidence; they afford the velocity of the wind much better than the use of light bodies, which scientific men have been under the necessity of rejecting, because their movements near the surface are complicated by the influence of innumerable whirlwinds and counter-currents.

In the year 1740, Franklin discovered that the hurricanes which so often ravaged the western parts of thè United States, propagated themselves in a course contrary to the direction in which they were blowing. Thus a hurricane from the *north-east* might begin at New Orleans; after a time it would arrive at Charleston: and two or three hours later it would reach Philadelphia; then after many hours it would be felt at New York, and at a still later period would reach the more northern towns of Boston and Quebec, in all this course proceeding backwards, for it always *blew from the northward.*

It results from this observation of Franklin that the hurricanes of America are *winds of aspiration.* Is not the same phenomenon produced in other places, and upon a very extensive scale? I say upon a great scale, because it appears to me beyond doubt that the land breeze which is regularly perceived during the night in certain seas, and the sea breeze which succeeds it in the morning, are likewise *winds of aspiration* or of *suction.*

Saussure, during his sojourn at the *Col-du-Géant*, was assailed by the wind of an extremely violent tempest, which intervals of the most perfect calm periodically interrupted. As the stormy winds suddenly changed their direction to the extent of 30 or 40 degrees, the illustrious philosopher of Geneva explained the strange intervals of calm which he witnessed, by supposing that sometimes the wind blew according to the direction of one or other of the Alpine ridges which protected his station at the *Col.*

This explanation of the intermission of the wind cannot be general, for Captain Cook has observed the same phenomenon in the open sea. Thus he writes in his second voyage. "When the ship was in 45° S. lat., and 28° 30′ E. long. from Paris, the night proved exceedingly stormy. The wind blew from the south-west in most violent squalls. In short intervals between these, the wind almost completely died away, and then recommenced with such fury that neither our sails nor rigging could stand it."

Captain Duperré has stated that he has sometimes seen the same thing: and this forms one curious subject of observation. It is also desirable to attend to the winds upon land, which often blow for whole days in the plains, if not with intervals of perfect calm, at least with changes in intensity, which Saussure estimated at the half, or at least two-thirds, of the usual intensity.

Meteorology and physiology have still much to gain from the zeal of travellers on the subject of the hot winds of the desert, known in Africa under the names of *Seimoum*, of *Kamsin*, and of *Harmattan*, winds which, in attaining the several islands of the Mediterranean, or the coasts of Italy, France, and Spain, become the *Sirocco.* The description which certain travellers have given of the effects of the seimoum are evidently exaggerated. It also appears evident that these effects, whatever they are, depend in a great degree upon their high temperature, and the extreme dryness which the shifting sands communicate to the atmosphere;

but it will be highly useful to complete, by accurate thermometrical and hygrometrical observations, the vague accounts with which we are now obliged to be satisfied. Burckhardt mentions that, during an invasion of the seimoum, he saw the thermometer at *Esné* in the shade rise to 131° Fahr., a temperature which would perfectly justify all the assertions of Bruce, if the British traveller had not added, that the air never continued in that state longer than a quarter of an hour.

In conclusion, is it true, as Burckhardt assures us, that the hues of the atmosphere where the seimoum prevails, the colours of the sun, whether red, yellow, bluish or violet, as mentioned by so many other travellers, depend on *the nature and colour of the ground*, whence the wind has raised the sand which it transports along with it?—*Arago.*

5. *Fall of Rain from a serene Sky.*—M. Wartmann has informed M. Arago, that, on the 31st day of May last, at two minutes after seven o'clock in the evening, there was a shower at Geneva, which lasted six minutes, the sky being perfectly clear at the zenith, and no cloud being visible in the neighbourhood of the place. This rain, whose temperature was lukewarm, fell vertically in drops, at first sufficiently large and numerous, and gradually became smaller and smaller till it ceased. The thermometer at the time indicated a temperature of 64°.5 Fahr. The day had been one of frequent showers and sunshine.

6. *Uncommon Atmospherical Refraction at Dover.*—When the dense fog which overspread the horizon here on Friday se'ennight had cleared away, about ten o'clock, the sky became so bright that one of the most imposing views of the opposite coasts presented itself that ever was witnessed from our shores. It was dead low water, which favoured the view, and it seemed as if a curtain had suddenly been withdrawn, exhibiting the whole line of the French coast as distinctly as if it had only been a few miles off. Calais was so plainly distinguishable that comparatively minute objects were plainly discernible. Boulogne piers were perfectly visible; the sails of the vessels in that harbour were observed outspread, and the whole of the villages along the coast seemed so close at hand that the spectator on Dover pier might fancy them as near as the martello towers immediately adjacent to Folkstone.—*September* 1838, *Dover Chronicle.*

HYDROLOGY.

7. *Submarine Currents.*—The temperature of the lower depths of the ocean, between the tropics, is from 22° to 25° Cent. below the lowest point at which seamen have observed the thermometer at the surface. Thus, this very cold bed at the bottom is not maintained by the precipitation of the superficial layers; and it hence seems impossible not to admit that submarine currents transport the water of the icy ocean to beneath the equator.

The consequence of this is important; and its reality is confirmed by the experiments which have been made in the Mediterranean. This *inland sea* cannot receive the cold currents which proceed from the polar regions in any other way than through the narrow Strait of Gibraltar. Hence, in the Mediterranean, the temperature of the greatest depths is never so low, *cæteris paribus*, as in the wide ocean. We may go further, and state that in no situation does the temperature of the bottom of the Mediterranean appear to descend lower than the mean temperature of the place. If this last circumstance be confirmed, it will follow that no portion of the icy stream coming from the poles enters the threshold, so to speak, of the Strait of Gibraltar. When Captain Durville sailed, some years since, in the first voyage of *L'Astrolabe*, I conceived it would be useful to examine if the phenomena of the ocean, as to the temperature of great depths, exhibited themselves in all their purity immediately to the westward of the Straits. The Academy then seconded my views; and upon this recommendation some observations of the required nature were made at a short distance from *Cadiz*. The result was an entire correspondence with those performed in the Mediterranean.

This curious fact seems to admit of two different explanations. It may be supposed either that the polar current is completely repelled by the submarine current which proceeds from the Mediterranean to the ocean, the existence of which seems scarcely doubtful. Or, it may be supposed also, that the bold projection of the southern coast of Portugal does not permit the current of cold water coming from the north to sweep round almost at a right angle to reach the parts which approach the mouth of the Guadalquiver. In this state of the question, the importance of thermometric soundings to the west and east of Cape St Vincent, will be apparent. Hence it appeared desirable that the Academy should recommend this series of observations to the Minister of Marine, and that a vessel should make a hydrographical survey of the coasts of Morocco, whose commander, *M. Bérard*, is already occupied in ascertaining the temperature of these seas at all depths, and with a degree of success which the scientific world will fully appreciate. Never did a more favourable occasion occur for the solution of the great problem of terrestrial physics whose elements we have now dwelt upon.—*Arago.*

8. *Water-Spouts.*—The connection of water-spouts with electro-magnetism, a favourite subject with some German naturalists, has been brought forward by Colonel Reid in his memoir on storms, read before the British Association, and we hope it will not be lost sight of. The double cones in water-spouts, one pointing upwards from the sea, the other downwards from the clouds, are interesting features in the phenomenon; these ought to be more particularly examined, and we should also ascertain whether the cloud above and the sea below revolve in the same direction with each other. We also suggest to those who may have an opportunity, to examine whether the rain which the spout projects in all directions is salt or fresh.

9. *Hot-Springs.*—If we admit, with some naturalists of our time, that hot-springs borrow their high temperature from the terrestrial strata which lie deep below them, many of these springs may then throw no trifling light upon the ancient thermometric state of the globe. An example, one of the most favourable that can be adduced, will exhibit the alliance of the two phenomena very distinctly.

In the year 1785, M. Desfontaines discovered, at some distance from Bona, in Africa, a hot-spring whose temperature was at 96° 3′ Cent. This spring was known to the ancients; for the ruins of baths, in the immediate neighbourhood, leave no doubt upon that point. This circumstance, combined with the temperature 96° 3′ Cent., leads, I conceive, to the consequence that during 2000 years, the temperature of the ground, in Africa, has not varied 4° Cent. For, let us admit for a moment, that during the 2000 years it had diminished 4° Cent.

the terrestrial strata whence the water now issues, must have possessed at the time of the Romans and Carthaginians a temperature of 100°.3 Cent., and consequently the water would have appeared at the surface in the state of steam, as in the *geysers of Iceland*, and not in the state of hot water only. But who can admit the existence of so extraordinary a phenomenon, when Seneca, Pliny, Strabo, Pomponius Mela, &c. are wholly silent regarding it.

Our argument seems liable only to one kind of difficulty; solutions will not boil at a temperature of 100° Cent,, as pure water does, and the difference will increase in the ratio of the quantity of saline matter which is dissolved. And this is the precise reason why new observations on the hot-spring in the neighbourhood of Bona are indispensable. It is on this account necessary to add to the determination of temperature, a chemical analysis of the water, an analysis which, however, can easily be made at Paris, upon specimens inclosed in bottles hermetically sealed. If, at the present time, the water of the spring reaches the surface very nearly saturated with the calcareous matters which it deposits, every difficulty will disappear, and an important problem in climatology will then be resolved.—*Arago.*

10. *Electricity near Waterfalls.*—In the year 1786, Tralles found, near the waterfall of the *Staubbach*, that the extremely fine rain, which was detached from it, gave manifest signs of negative electricity. The *Reichenbach* also presented the same phenomena. Shortly after, Volta verified the accuracy of the observation of Tralles, not only at the waterfall of *Pissevache*, but also every where when a fall of water, however insignificant, gave occasion, by the intervention of the wind, to the dispersion of the minute globules. It appeared to him, as it did to Tralles, that the electricity was always negative. The philosopher at Berne at first attributed the electricity of the watery vapour, with which all great cascades are surrounded, to the friction of the globules upon the air; but shortly afterwards he saw, along with Volta, the true cause of this electricity in the evaporation which these same globules experienced in the act of falling. This explanation has lately been combated by Professor *Belli.* Without denying that

evaporation may have some effect upon the phenomena, M. Belli ascribes the principal influence to the action which atmospheric electricity must exercise upon running water. Water, he says, will be by influence, by induction, in a state of negative electricity, when the atmosphere is, as usually happens, charged with positive electricity. At the moment in which this water is divided into a thousand globules, it cannot fail to convey the electricity, with which the impulsion of the atmosphere had impregnated it, to every object which it encounters. The theory of Professor Belli is susceptible of a proof which, at once, *will demonstrate* either its truth or its inaccuracy. If it be true, the electricity of the vapour with which waterfalls are surrounded, will not be always of the same kind; it will be negative if the atmosphere is positive; and, on the contrary, it will be found positive when the clouds are negative. It must, then, be observations made in the time of storms, and not under a serene sky, which will enable us to ascertain the comparative merits of the theories of Volta and M. Belli.—*Arago.*

11. *Tides in the Mediterranean.*—The theory of tides, borrowed from the principle of universal attraction, can leave no doubt as to its general truth. What it still wants in simplicity and accuracy will be afforded by geometry. Observers, at the same time, have a great field of inquiry before them, in the local circumstances which considerably modify the tide-tables of different ports, and the changes in the height of the water, where it is not easy to state what is the influencing circumstance, and what its mode of action. Are there any sensible tides in the Mediterranean properly so called? To this question some individuals respond yes; as it concerns, for example, the harbour of *Bouc;* but the calculations on which they rest give a contrary result. According to some researches made at Naples, in the year 1793, there was there a very observable tide to *the extent of about a foot* in the straight canal which is called the river *Styx*, and which establishes a communication between the port of *Misénc* and the *Marc-Morto.* Blagden regarded these data so certain that he even proceeded to deduce from them the hour of its establishment in the Bay of Naples. These observations deserve to be repeated in different parts of the coast of Algiers; and the want of success in some harbours ought not to prove discouraging as it regards

others. If we had been restrained by the remark, so often repeated, that the Mediterranean is too confined to allow tides to be observed, we should not now have known that they are quite sensible in the *Adriatic* ; and we should have been ignorant that at *Chioggia* and *Venice* they rise more than a yard.—*Arago.*

GEOLOGY.

12. *Geology of the Zahara Desert.—From M. Elie de Beaumont's Instructions to the Scientific Commission to Algiers.*—M. Rozet, from the observations he has himself made upon the tertiary strata in the neighbourhood of Algiers and Medeya, and according to the information he has received from the celebrated traveller M. Réné Caillié, thinks it highly probable that the tertiary formation of the district of Algiers constitutes the soil of the Great Desert of Zahara. He considers that sandstones and the tertiary limestones will be met there in horizontal beds, covered with great masses of sands, which will be found to be nothing more than those which very frequently appear at the upper part of the sub-Atlantic tertiary formation,* with this difference, that to the south of the Great Atlas the sands will have acquired an exceedingly great development. The argillaceous marls, which, according to M. Rozet, must exist at the lower part of the tertiary series in the Zahara Desert, as well as near the Atlas, as they readily retain water, make it highly probable that abundant springs would appear upon boring. Draw-wells might thus be easily established. Mr Shaw even mentions, that in the villages of *Wad-reag* they are in the habit of procuring water by machinery, which reminds us of our Artesian wells, or spouting fountains. This fact is alluded to by M. Arago in his able article upon *Les Puits Artésiens* in the *Annuaire* for 1834.† This possibility of establishing spouting-fountains, not only in the Zahara Desert, but also in many places throughout the Algerine district, not excepting many near the coast, is so important, that it deserves to be particularly recommended to the attention of those who superintend the geological department of the commission. Again, the sands of the Desert of Zahara, and of those much smaller deserts

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* Rozet names the Tertiary deposits of Algiers sub-Atlantic.

† A translation of this Memoir is published in vol. xviii. of Edinburgh New Philosophical Journal.

which lie between the mountains of Algiers, should be examined, with an especial relation to the sands of downs; so that the differences which exist between these two varieties of sands may be ascertained, both as it regards their present circumstances and their respective origins. It has long been known that the sands of the deserts near Egypt contain trunks of silicified trees. Some indications would lead to the supposition, that these may also be found in the deserts of Algiers; and it would be interesting to come to a definite conclusion on this point, as the presence of such silicified trees would tend to establish the tertiary origin of the sands. A great sandy desert, viz. that of Anga, separates, on the east, the regency of Algiers from the kingdom of Fez. This desert is undoubtedly analogous in its geological origin to the Great Desert of Zahara, and it would be of much easier access to the members of the scientific expedition than the other. One of the most curious facts which the deserts of Africa and Asia present is, that their soil is frequently salt. Chloride of sodium is spread over the soil of Barbary in surprising abundance. According to M. Desfontaines, the ground, throughout nearly the whole extent of the regency of Tunis, is impregnated with so great a quantity of sea-salt, that the majority of the springs are salt-springs. It is by no means rare to observe, when the summer heat has evaporated the stagnant waters of low places, that a considerable extent of the surface is encrusted with the salt which had been dissolved and accumulated by the winter's rains. These plains are usually denominated *Sibkah*, in other words, salted earth. In winter they are usually covered with water, and then appear like so many great lakes; but when dried in summer, they very much resemble great bowling-greens covered with beautiful turf. Some of these *Sibkahs* have a hard and solid bottom, without any mixture of earth or gravel, which retains the salt, and forms a crystalline crust after the rains. Saline deposits of this sort occur near *Arzew*, and in several other localities. It would be interesting to examine the relations which exist between the salt that is thus superficially spread over the surface, and the masses of rock-salt which also exist in the district of Algiers. Shaw considers them nearly allied, for he remarks that the salt of the Lake Marques, which is also called *Bahirah-Pha-*

raonne, and of some other less considerable plains of the same nature, resemble rock-salt, both in taste and quality.

13. *Fossil Organic Remains in Sutherland, and Fluor-Spar in the Ord of Caithness.*—A letter from one of our pupils, Mr Clarke junior, of Eriboll, informs us that he has found organic remains in a quartz-rock connected with the limestone of Sutherland. This is, we believe, the second time petrifactions have been found in a part of Scotland, so interesting from its display of transition-rocks. Another of our pupils, Mr R. H. Cunningham, has discovered veins of fluor-spar in the granite of the Ord of Caithness.

ZOOLOGY.

14. *Hydrophobia in Mohammedan Countries.*—M. Serres, in his instructions to the medical department of the proposed African expedition, makes the following remarks concerning hydrophobia:—"Is it true that dogs in warm countries, and especially among Mussulmans, are less subject than elsewhere to hydrophobia? If they verify this fact, the Commission ought also to inquire into its cause; because in France it is almost always during warm weather, or after long droughts, that the disease appears among these animals. Without anticipating the explanation which may be supplied, we may here mention a fact which possibly may bear upon it. It has been remarked in Paris, that, since the establishment of running fountains, hydrophobia has become less frequent; and this has been attributed to the limpid streams which run down the streets, and to the facility which this affords to the dogs at all times to quench their thirst. For it is known that dogs drink frequently, and at the same time will but rarely partake of muddy or soiled water. It has moreover been observed, that the hydrophobic cases which have occurred in later years have been witnessed among dogs belonging to the precincts of the town, where the ponds, fed with rain water, are almost wholly dried up in summer. It has also been noticed that the time of their amours is that in which they are most liable to the complaint. If the assertion of the comparative rarity of the disease among Mussulmans be correct, may not the cause be partly owing to the great care which these people take of their dogs, and partly to the abundance of water which the daily prescribed ablutions of

the Koran requires? May not these circumstances, connected with the observation made at Paris, and with the not less important one, that the disease scarcely ever originates in large kennels, direct us to the prevention of the malady among dogs, and consequently among men? For it is to the *prevention* of this complaint that we should apply all our energies, since the unsuccessful results of the experiments which have been made in the Hotel-Dieu during the last twenty-five years, almost lead us to despair that we shall ever discover any remedy whereby we may arrest its fatal result."—At the next meeting of the Academy, M. Larrey supplied the following note:—"Hydrophobia, though common in hot climates, is not seen in Egypt, and but rarely upon the northern shores of Africa. So far as Egypt is concerned, the absence of the disease, undoubtedly depends upon the peculiar variety of the dog which prevails in that country, and upon its habits and modes of life. In all these particulars it is very nearly allied to the fox; and hence it is remarked that they appear always in a state of inaction. The truth is, they always remain quiet in the shade during the day, close to the cisterns of cold water, which are filled every twenty-four hours, and preserved in great order by the inhabitants. During the nights again, these animals are all alive; they are also but seldom in heat, and but for a short time. If we found a vast number of these animals on our arrival in Egypt, this was owing to their being held in so much respect in the country, as are many other animals, and to their never being destroyed. They never enter into any of the houses. During the day they lie quiet, as stated, in the confines of the street, and during the night they range into the country, and devour the carcasses of those animals which have not been interred. They are gentle and peaceable, and seldom fight among themselves. It is possible that all these circumstances raise these animals above the attacks of hydrophobia. Another remarkable circumstance, however, is this, that the camels in Egypt, as M. Magendi has observed of the dogs in Paris, are subject to a variety of the complaint during the rutting season. They are then liable to a copious thick white discharge; they bellow unceasingly, they do not drink, and appear to have a horror for water; they then, too, pursue other quadrupeds, and even man, for the purpose of biting; they become very lean, and their hair stands on end and falls off. They are often feverish; and if in this state they are in any way violently excited, after some days of severe suffering they die in convulsions. When they are labouring under this disease, the bites of these animals are very dangerous. To prevent its fatal effects, the camel-drivers are in the habit of muzzling them at these times, and are scrupulously cautious lest they be bitten.

15. *Experiments upon the Torpedo.*—M. Matteucci, in a letter addressed to M. Dulong, announces that new experiments which he has recently made upon the torpedo completely confirm the results he had previously obtained respecting the unequal power of different parts of the brain in the production of shocks; thus the hemispheres of the cerebrum may be touched, wounded, and even removed without a discharge taking place. Again, it may be obtained, but only whilst the animal is very lively, from the *thalami nervorum opticorum (couches optiques)* situated between the hemispheres of the cerebrum and cerebellum. As to the cerebellum *(quatrième lobe)*, it cannot be touched without producing a discharge; the effect is still produced, some time even after the death of the animal, and when this part is removed all discharge ceases.

NEW PUBLICATIONS.

1. *Zoology of the Voyage of H. M. S. Beagle, under the command of Capt. Fitzroy, during the years* 1832 *to* 1836. *No. II. of Part II. Recent Mammalia.* By George R. Waterhouse, Esq. Smith, Elder & Co., London.

This part 2d of No. II. of the Recent Mammalia of the Beagle Voyage contains descriptions and histories of the *Canis Magellanicus*, *fulvipes*, and *Azaræ*; of *Felis Yagouarondi* and *pajeros*; of *Delphinus Fitz-Royi*; also remarks on the Guanaco of the aborigines of Chili; and an account of the *Cervus campestris*. There are six very interesting and beautifully executed coloured engravings of nine species of Muridæ, to be described in a future part.

No. 1st of part 3d, which has just reached us, contains accurately drawn and beautifully coloured representations of birds, by Mrs and Mr Gould; with notices of the habits and ranges of the tribes and species, by Mr Darwin. But our particular notice of this part must be delayed until our next number.

2. ***Illustrations of the Zoology of South Africa, &c. &c.*** By Andrew Smith, M. D., Surgeon to the Forces, and Director of the Expedition into the Interior of Africa. In 4to Parts. To be published periodically by Smith, Elder & Co., London.

This is another of those important and elegant original works on natural history at present in the course of publication. Dr Smith, the chief author and conductor of the Illustrations, is a naturalist of established reputation, and a traveller distinguished for zeal, activity, and judgment. Being stationed at the Cape of Good Hope as a medical officer in the military service, Dr Smith had ample opportunities of indulging his taste for natural history. Fortunately for science, he was selected by the *Cape of Good Hope Association for exploring Central Africa*, as chief of an expedition sent out by that Society, for the purpose of extending our knowledge of Southern Africa to the north of the Cape district. The expedition, consisting of thirty-four persons, after an absence of nineteen months, and penetrating as far as 23° 28′ south latitude, returned to Cape Town, with an extensive and valuable collection of objects of natural history. Descriptions and figures of the new and more interesting species of animals are to be laid before the public in the "Illustrations of the Zoology of Southern Africa."

The work will consist of pictorial illustrations of between two and three hundred subjects of the animal kingdom, all of which have been collected to the south of 23° 38′ of south latitude, and will comprise, first, and principally, unknown animals; secondly, animals known, but not yet figured; and, lastly, such as have been imperfectly figured, but of which Dr Smith is in possession of accurate drawings. The entomological part of the work will be written by W. S. Macleay, Esq.; the rest of the descriptions by Dr Smith, who will add a summary of African zoology, and an inquiry into the geographical range of the species in that quarter of the globe.

Part 1st contains ten beautifully drawn and coloured plates, and descriptions of the following animals:

Pl. 1. Rhinoceros Keitoloa.—This colossal animal, although nearly allied to the Rh. bicornis, is evidently a distinct species.

Pl. 2. Rhinoceros Bicornis, *Linn.*—On this species Dr Smith has the following interesting remarks. "The present species, under the name *Rhinoster*, has been familiarly known to the colonists of the Cape of Good Hope ever since 1652. In that year, when the Dutch first formed their settlement on the shores of Table Bay, this animal was a regular inhabitant of the thickets which clothed the lower slopes of Table Mountain. The abandonment of those spots by this animal, as a measure of safety, probably constituted the commencement of a forced migration, which has continued to extend ever since, and which has led not only to the disappearance of the species from the districts within the present colonial limits, but also in a great measure to its removal from countries beyond those limits, as far as hunters, efficiently armed, are accustomed to resort. If a system, such as has hitherto prevailed, continues to exist, and the larger animals persevere in flying to avoid the effects of fire-arms, the time may arrive when the various species which formerly may have been scattered, each in a particular locality of a large continent, will be huddled together; and indeed an advance towards that period is in progress, as may be inferred from the concentration which is at present taking place in the interior of Southern Africa. Until lately, the present was the only species of the genus which was known to be receding from its native country, but of late another has been led to a like course; and the *Rhinoceros Simus*, which, but a few years ago, was common in the neighbourhood of Latakoo, has, since the more general introduction of fire-arms into that country, almost entirely ceased to approach within a hundred miles of it. From a consideration of the various facts which we have collected in relation to the species now under consideration, and which we shall detail more at length elsewhere, we feel disposed to regard it to a certain extent as a prisoner in the country it now inhabits, and are inclined to believe the southern extremity of the continent, and the country along the western coast towards Benguela to have once formed a favourite residence."

Pl. 3. Falco semitorquatus, *Smith*.—Of the Falcon tribe, eight species appear to be natives of Southern Africa, of these one is our Falco peregrinus," or Peregrine falcon, another the Hobby, Falco subbuteo; and two are new species, viz. the present and the Falco rupicoloides of Smith.

Pl. 4. Chizærhis concolor, *Smith*.—This beautiful bird is a new species of the genus Chizærhis of Wagler.

Pl. 5. Pterocles gutturalis; and, Pl. 6. Otis ruficrista, *Smith*. Both new species. Pl. 7. Sternotherus sinuatus, *Smith*. This new species of the Tortoise tribe was met with in greatest abundance between 24° and 25° N. lat.

Pl. 8. Varanus albogularis, *Daud.* This reptile, which is from four feet to five feet in length, occurs but rarely within the limits of the Cape Colony. It is usually discovered in rocky precipices, or in low stony hills, and when surprised seeks concealment in the chinks of the former, or in the irregular cavities of the latter; and when any irregularities exist upon the surface of the rocks or stones, it clasps them so firmly with its toes that it becomes a task of no small difficulty to dislodge it, even though it can be easily reached. Under such circumstances, the strength of no one man is able to withdraw a full-grown individual; and I have seen two persons required to pull a specimen out of a position it had attained, even with the assistance of a rope fixed in front of its hinder legs. The moment it was dislodged it flew with fury at its enemies, who by flight only saved themselves from being bitten. After it was killed, it was discovered that the points of all the nails had been broken previously, or at the moment it lost its hold. It feeds upon frogs, crabs, and small quadrupeds.

Pl. 9. Bucephalus viridis, *Smith.* This beautiful snake, but once met with by Dr Smith, he considers a distinct species of his genus Bucephalus.

Pl. 10. Echinorhinus obesus, *Smith.* This plate represents a species of a new genus of the Shark tribe.

The second and third numbers, equally interesting, will be considered in our next.

3. *Arboretum et Fruticetum Britannicum, or the Trees and Shrubs of Britain, native and foreign, hardy and half hardy, pictorially and botanically delineated, and scientifically and popularly described; with their propagation, culture, and management, and uses in the arts, in useful and ornamental plantations, and in landscape gardening; preceded by a historical and geographical outline of the trees and shrubs of temperate climates throughout the world.* By J. C. Loudon, F.L. and H.S., &c. In eight volumes 8vo. Illustrated by above 2500 wood-cuts, and 297 figures of Trees and Shrubs. Longman, Orme, Brown, Green and Longmans, London.

We have many good books on gardening, but, with exception of Evelyn's Sylva, now antiquated, few or none on the natural and economical history of the trees and shrubs of Great Britain and Ireland. Mr Loudon (by the bye, a very remarkable man) has, by the publication of these important, interesting, very useful, generally accurate, and beautifully illustrated volumes, contributed to the literature of this country a work not surpassed by any within the range of our reading. To the professional forester, the improver and embellisher of estates or pleasure grounds, Mr Loudon's work will afford on all occasions useful and accurate information, for there is not a subject connected with these operations, that is not to be found amply discussed there; even the amateur will consult the Arboretum with great pleasure and much advantage. We do not say more than the work merits, if we add, that it should form the manual of the professed forester and embellisher of grounds, and find a place in all our public libraries, and the library of every country gentleman.

4. *Dictionary of Arts and Manufactures, and Mines; containing a clear exposition of their principles and practice.* By A. Ure, M.D., F.R.S., &c. Part I. 8vo. pp. 120. Longman & Co., London.

We expected from a chemist of Dr Ure's celebrity, a work of a superior cast, and in this we have not been disappointed. On perusing the number now before us, we find it promises to meet the high expectations of the public, and to increase the reputation of the author. The chief articles in this Part are the following:—Acetic Acid, Alcohol, Alum, Anchor, Assay, Baths, and Beer. Where necessary, the descriptions of apparatus and processes are amply illustrated by engravings. It is the intention of Dr Ure to complete the work in ten monthly parts, and to illustrate it with upwards of one thousand engravings in wood.

5. *A History of the British Zoophytes.* By George Johnston, M.D., Edin. F.R.C.S., R.M.S.Ed. With 44 plates and 51 figures. W. H. Lizars, Edinburgh. 8vo. pp. 341.

Dr Johnston has done a great service to the Fauna of Britain, by the publication of this valuable volume. Since the time of Ellis the zoophytes of our country have occasionally engaged the attention of observers, and their observations have been laid before the public through various channels. Now, for the first time since the publication of the celebrated "Essay towards a natural history of Corallines, and other productions of the like kind, commonly found on the coasts of Great Britain and Ireland, published in 1755," Dr Johnston has brought together in a separate volume, and with much additional original information, a full natural history of our British Zoophytes.

The work is divided into five parts. Part I. contains an amusing history of Zoophytology; a general account of the structure and physiology of zoophytes; and their classification, a dry subject, which is made readable by the skill of our author. He divides the British Zoophytes into the following sub-classes and orders, viz.:—Sub-class I. Radiated Zoophytes. Order 1. Hydroida. Order 2. Asteroida. Order 3. Helianthoida. Sub-class II. Molluscous Zoophytes. Order 4. Ascidioida. Parts II. III. IV. and V. are appropriated to the natural history of the species.

Each of these four parts is devoted to the consideration of one of the orders, with all its families, genera, and species. We do not here, as is too often the case in systematic works, meet only with dry details; on the contrary, great accuracy of discrimination is combined with valuable anatomical and physiological observations, and beautiful views in natural history.

The accompanying plates do honour to the artist, without whose assistance this work would in all probability never have seen the light. Our author was fortunate indeed in having the aid of such a pencil as that of Mrs Johnston.

A delightful feature in the "History of the British Zoophytes," cannot pass unnoticed; we allude to the interesting biographies of known and also of unknown, but most meritorious observers, agreeably distributed throughout the work. They recall to our imagination the times which have past—the Linnæan epoch—and give an antique and fascinating air to this classical work, ere long to be in the hands of every British naturalist.

List of Patents granted in Scotland subsequent to 11th June 1838.

1. To **Miles Berry** of the Office for Patents, Chancery Lane, in the county of Middlesex, patent agent and mechanical draftsman, in consequence of a communication from a foreigner residing abroad, for an invention of "certain improvements in the means of economizing heat and fuel in furnaces or closed fire-places."—15th June 1838.

2. To **David Cheetham** junior of Hollins Mill, Staley Bridge, in the county of Chester, cotton-spinner, for an invention of "certain improvements in the machinery applicable to the preparation of cotton and other fibrous substances for the purposes of spinning."—15th June 1838.

3. To **Edmund Butler Rowley** of Chorlton upon Medloch, in the parish of Manchester and county of Lancaster, surgeon, for an invention of "certain improvements applicable to locomotive engines, tenders, and carriages to be used upon railways, and which improvements are also applicable to other useful purposes."—19th June 1838.

4. To **William Sanford Hall** of Strathearn Cottage, Chelsea, in the county of Middlesex, Lieutenant (on half-pay) in her Majesty's royal army, for an invention of improvements in paddle-wheels."—21st June 1838.

5. To **Joseph Rock Cooper**, of Birmingham, in the county of Warwick, gunmaker, for an invention of "improvements in fire-arms."—21st June 1838.

6. To **John William Fraser** of Arundel Street, Strand, in the county of Middlesex, for an invention of "improvements in diving, or descending and working in water, and for raising or floating sunken and stranded vessels, and other bodies."—21st June 1838.

7. To **Joshua Taylor Beale** of No. 11 Church Lane, Whitechapel, in the county of Middlesex, engineer, for an invention of "certain improvements in and additions to his former invention, known by the title of a lamp applicable to the burning of substances not hitherto usually burned in such vessels or apparatus."—26th June 1838.

8. To **Edward Cobbold** of Long Melford, in the county of Suffolk, clerk, Master of Arts, for an invention of "certain improvements in the manufacture of gas for affording light and heat, and in the application of certain products thereof to useful purposes."—27th June 1838.

9. To **Stephen Geary** of Hamilton Place, New Road, in the county of Middlesex, architect, for an invention of "improvements in the preparation of fuel."—27th June 1838.

10. To **William Gossage** of Stoke Prior, in the county of Worcester, manufacturing chemist, for an invention of "certain improvements in manufacturing sulphuric acid."—29th June 1838.

11. To **Francis Thorpe**, of Knaresborough, in the county of York, flax-spinner, for an invention of "certain improvements in machinery or apparatus for heckling, preparing or dressing hemp, flax, and other such like fibrous materials."—29th June 1838.

12. To **Peter Fairbairn**, of Leeds, in the county of York, machine-maker, for an invention of "certain improvements in the machinery or apparatus for roving, spinning, doubling, and twisting cotton, flax, wool, silk, or other fibrous substances."—6th July 1838.

13. To **Henry Davies** of Stoke Prior, in the county of Worcester, engineer, for an invention of "certain improved apparatus or machinery for obtaining mechanical power, also for raising or impelling fluids, and for ascertaining the measure of fluids."—11th July 1838.

14. To **Edward Davy**, of Fordton, near Crediton, in the county of Devon, merchant, for an invention of "certain improvements in saddles and harness."—11th July 1838.

15. To **Joseph Frederick Burnett** of St Mary, at Hill, in the city of London, ship insurance agent, and **Hippolyte Francois Marquis de Bouffet Montauban**, colonel of cavalry, now residing in Sloane Street, Chelsea, in the county of Middlesex, in consequence of a communication from a foreigner residing abroad, for an invention of "certain improvements in the manufacture of soap."—11th July 1838.

16. To William Rattray of Aberdeen, North Britain, manufacturing chemist, for an invention of "certain improvements in the manufacture of the preparations called gelatine size and glue."—12th July 1838.

17. To Henry Count de Crony, of the Province of Picardy, in the kingdom of France, now residing at No. 14 Cambridge Street, Edgeware Road, in the county of Middlesex, for an invention of "a new and improved method of filtration," communicated partly by a foreigner and partly invented by himself.—13th July 1838.

18. To Francis Pope of Wolverhampton, in the county of Stafford, fancy iron-worker, for an invention of "certain improvements for making or manufacturing pins, bolts, nails, and rivets, applicable to various useful purposes."—13th July 1838.

19. To Bennet Woodcroft of Mumps, in the township of Oldham, in the county of Lancaster, gentleman, for an invention of "improvements in the construction of looms for weaving various sorts of cloths, which looms may be set in motion by any adequate power."—19th July 1838.

20. To Charles Bourjot, of Coleman Street, in the city of London, merchant, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "improvements in the manufacture of iron."—19th July 1838.

21. To Jean Leandre Clement of Rochfort, in the kingdom of France, but now of Jauney's Hotel, Leicester Square, in the county of Middlesex, gentleman, for an invention of "improvements in apparatus for ascertaining and indicating the rate of vessels passing through the water."—19th July 1838.

22. To Nicholas Thomas Raper of Greek Street, Soho, in the county of Middlesex, gentleman, for an invention of "improvements in rendering fabrics and leather water-proof."—19th July 1838.

23. To Luke Hebert of High Street, Camden Town, in the county of Middlesex, patent agent, in consequence of a communication from a foreigner residing abroad, for an invention of "a new and improved method or methods of uniting or soldering metallic substances."—19th July 1838.

24. To Joseph Bennet of Turnlee, near Glossop, in the county of Derby, cotton-spinner, for an invention of "certain improvements in machinery for carding wool, cotton, flax, or other fibrous substances, which are or may be carded, part of which improvements are also applicable to machinery for drawing, doubling, roving, and spinning such fibrous substances as are or may be subjected to their operations."—26th July 1838.

25. To Richard March Hoe, late of New York, in the United States of America, but now residing at the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, in consequence of a communication made to him from Dr H. H. Sherwood, of New York aforesaid, a foreigner residing abroad, for an invention of "a new or improved instrument or apparatus for ascertaining and determining the latitude or longitude of any place, or the situation of ships or other vessels at sea, and the dip or variation of the magnetic needle, which new or improved instrument he intends to denominate Sherwood's Magnetic Geometer.—26th July 1838.

26. To Richard March Hoe, late of New York, in the United States of America, but now residing at the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, for an invention of "certain improvements in machinery or apparatus for grinding and polishing metal surfaces."—28th July 1838.

27. To William Barnett of Brighton, in the county of Essex, iron-founder, for an invention of "certain improvements in the production of motive power, and in the manufacture of iron."—31st July 1838.

28. To Richard Badnall of Cotton Hall, in the county of Stafford, gentleman, for an invention of "a certain improvement in the manufacture of carpets and other similar woven fabrics, which improvement is effected by the introduction of a certain article of commerce not hitherto so employed or used in such manufactures."—31st July 1838.

29. To Richard Treffry of Manchester, in the county of Lancaster, chemist, for an invention of "certain improvements in the methods for preserving certain animal and vegetable substances from decay, and also in

the apparatus for and mode of impregnating substances to be preserved."—6th August 1838.

30. To Robert Sandiford of Tottington-lower-end, in the county of Lancaster, block-printer, for an invention of certain "improvements in the art of block-printing, and in certain arrangements connected therewith."—7th August 1838.

31. To John Thomas Belts of Smithfield Bars, in the city of London, rectifier, in consequence of a communication from a person residing abroad, for an invention of "improvements in the process of preparing spirituous liquors in the making of brandy."—9th August 1838.

32. To Henry Bessemer of City Terrace, City Road. in the parish of St Luke, Old Street, and county of Middlesex, engineer, for an invention of "certain improvements in machinery or apparatus for casting printing-types, spaces, and quadrats, and the means of breaking off and counting the same."—9th August 1838.

33. To Peter Fairbairn of Leeds, in the county of York, machine-maker, in consequence of a communication from a foreigner residing abroad, for an invention of "certain improvements in looms for weaving ribbons, tapes, and other fabrics."—10th August 1838.

34. To Sir James Caleb Anderson of Buttevant Castle, in the county of Cork, Baronet, for an invention of "certain improvements in locomotive engines, which are partly applicable to other purposes."—18th August 1838.

35. To David Cheetham junior of Staley Bridge, in the county of Chester, spinner, for an invention of "certain improvements in the means of consuming smoke, and thereby economizing fuel and heat in steam-engine or other furnaces, or fire-places."—22d August 1833.

36. To James Robinson of Huddersfield, in the county of York, merchant, for an invention of "an improved method of producing by dyeing various figures or objects of various colours in woollen, worsted, cotton, silk, and other cloths."—22d August 1838.

37. To Charles Dod of 21 Craven Street, Strand, in the county of Middlesex, gentleman, in consequence of a communication made to him by a foreigner residing abroad, for an invention of "certain improvements in the construction of railway tram-roads, and in the structure of the carriages to be used in the said railways or tram-roads, and also of certain apparatus applicable to the cleaning and preserving of railways and tram-roads."—23d August 1838.

38. To Arthur Dunn of Stamford Hill, in the county of Middlesex, gentleman, for an invention of "certain improvements in the manufacture of soap."—24th August 1838.

39. To Ambroise Ador, late of Leicester Square, in the county of Middlesex, but now of 29 Rue de Faubourg Montmartre, in the city of Paris and kingdom of France, chemist, for an invention of "certain improvements on lamps or apparatus for producing or affording light."—28th August 1838.

40. To Charles Phillips of Chipping Norton, in the county of Oxon, surgeon, for an invention of "improvements in apparatus or machinery for punching, bending, cutting, and joining metal, and for holding or securing metal to be punched, bent, cut, or otherwise operated on, parts of which machinery are adapted to perform some of these operations on other materials."—31st August 1838.

41. To Job Cutler of Lady Pool Lane, Sparkbrook, in the borough of Birmingham, in the county of Warwick, gentleman, and Thomas Gregory Hancock, of Prince's Street, in the borough of Birmingham aforesaid, mechanic, for an invention of "an improved method of condensing the steam in steam-engines, and supplying their boilers with the water thereby formed."—31st August 1838.

42. To Charles Fitton, woollen manufacturer, and George Collier, mechanic, both of Cumberworth Half, Wakefield, in the county of York, for an invention of "improvements in power-looms."—6th September 1838.

43. To Charles Hancock of Grosvenor Place, Hyde Park Corner, in the county of Middlesex, animal painter, for an invention of "certain improved means of producing and applying figured surfaces, sunk and in relief, and of printing therefrom, and also of moulding, stamping, and embossing."—13th September 1838.

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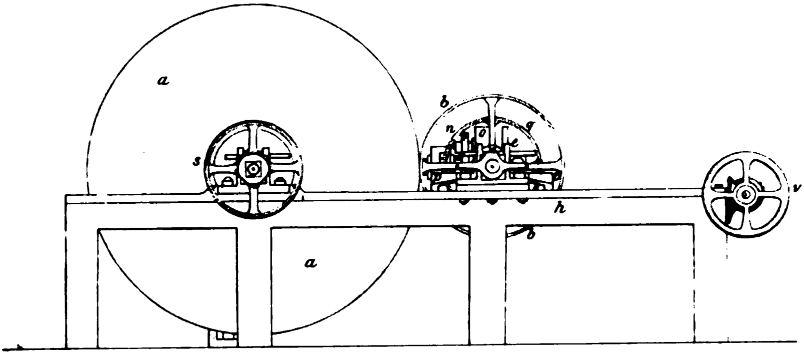

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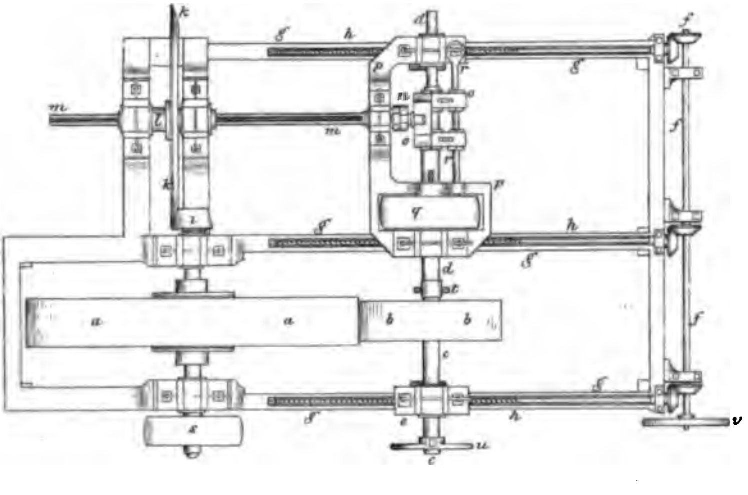

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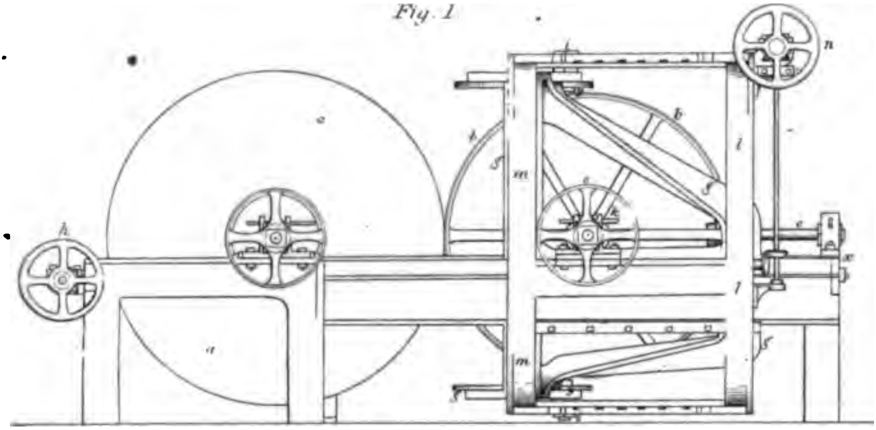

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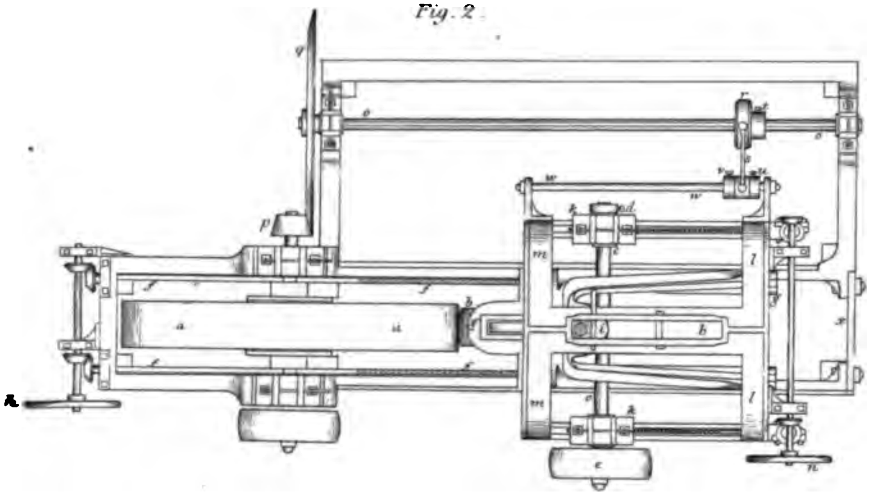

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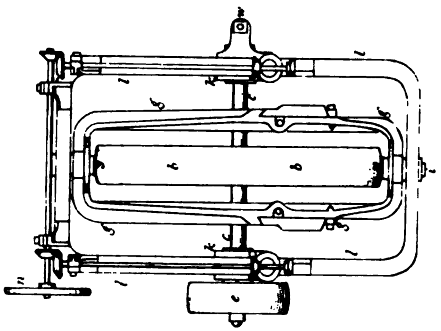

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